

THE
BRITISH JOURNAL
OF
PSYCHOLOGY

GENERAL SECTION

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VOLUME XXVIII, 1937-38

CAMBRIDGE
AT THE UNIVERSITY PRESS

1938

THE
BRITISH JOURNAL
OF
PSYCHOLOGY
GENERAL SECTION

PRINTED IN GREAT BRITAIN

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BRITISH PSYCHOLOGICAL SOCIETY

RULES

NAME AND OBJECTS

1. This Society shall be called the British Psychological Society.
2. The Society is instituted for promoting the co-operation of those interested in the various branches of Psychology.
3. The Society shall be responsible for the publication of *The British Journal of Psychology*.

MEMBERS AND SECTIONS

4. The Society shall consist of Honorary Members, Ordinary Members, Branch Associates and Sectional Associates.
5. Persons of scientific distinction who have contributed to the advancement of Psychology shall be eligible for election as Honorary Members.
6. Honorary Members shall be entitled to all rights and privileges of membership.
7. Honorary Members may be elected from time to time on the nomination of the Council at any General Meeting of the Society.
8. The Council shall be empowered to institute Sections of the Society, each Section being concerned with a special branch or aspect of Psychology.
9. Election to a Section or Sections shall *ipso facto* constitute election to the Society as a whole and entitle those so elected to receive notice of and to take part in all General Meetings of the Society in addition to the meetings of the Section or Sections to which they belong.
10. Sections shall have the right of electing Sectional Associates who shall receive notice of and shall be permitted to attend all Meetings of the Section or Sections to which they belong. Such Sectional Associates shall however take no part in the Government of the Society or the Sections, and they shall not receive *The British Journal of Psychology*.
- 10 a. Transfer from Sectional Membership to Sectional Associateship may take place on January 1st only, and after not less than six months' notice from the Member of his wish to transfer.
11. The procedure as regards the election of Sectional Associates shall be the same as that in the case of Members.
12. Every Candidate for election to the Society shall be recommended by at least two Members who shall be prepared to furnish

information as to the Candidate's qualifications for membership. No Candidate shall be submitted for election to the Society who has not first been approved by the Council or by a Sectional Committee.

13. A Candidate who has been approved by the Council or by a Sectional Committee shall be nominated for election at the next General Meeting of the Society, or at the next Meeting of the Section respectively.

14. One full week before the Meeting at which the ballot is to take place, the Secretary shall send to each Member a balloting paper containing the names and addresses of the Candidates and the names of their proposers.

15. Any Member unable to attend a Meeting at which a ballot is to take place can vote by sending his balloting paper to the Secretary.

16. One adverse vote in five shall exclude.

17. To vote *for* a Candidate, a Member must prefix a cross to the Candidate's name; to vote *against* a Candidate, he must erase the Candidate's name. Members who wish to record a vote neither for nor against a Candidate must leave the name untouched.

18. Any Member whose subscription remains unpaid for two years may, after receiving due notice, be removed by a resolution of the Council from membership of the Society.

19. Whenever it shall be proposed to remove a Member from the Society for any reason other than failure in payment of subscription, the matter shall be brought as a formal resolution before the Council or one of the Sectional Committees (whichever it shall most concern). In the event of the resolution being passed, a ballot shall be held for the removal of the said Member at the next General Meeting of the Society, or at the next Meeting of the Section respectively. Should two-thirds of the Members voting vote in favour of the removal of the Member, that Member shall be removed from the Society.

20. If at any time it is desired to abolish any Section or Sections, to subdivide any Section or Sections, or to amalgamate two or more Sections, a Special Meeting of the Section or Sections concerned may be called for this purpose according to the conditions laid down in Rule 44. Resolutions passed at any such Meeting shall be reported by the Sectional Secretary or Secretaries concerned to the next Meeting of the Council. Should the Council approve of such resolutions, such resolutions shall immediately take effect. But should the Council formally express disapproval of any or all of the said resolutions, these resolutions shall be considered by an Extraordinary General Meeting of the Society sum-

moned in accordance with the conditions laid down in Rule 44 and the decision of this Meeting shall be final.

In the case of the desire to abolish any Section or Sections, the following alternative procedure may be followed. An Extraordinary General Meeting of the Society shall be summoned in accordance with the conditions laid down in Rule 44, and any resolution passed at this Meeting regarding the abolition of the said Section or Sections shall be final.

OFFICERS, COUNCIL AND SECTIONAL COMMITTEES

21. The business of the Society as a whole shall be conducted by a Council consisting of the President, Deputy President, Vice-Presidents, Treasurer, General Secretary, Librarian, Editors of *The British Journal of Psychology*, one representative each of every Section and Branch to be nominated by the Sectional and Branch Committees, and of six Ordinary Members, and not more than two members co-opted by the Council.

22. The President and Deputy President of the Society shall hold office for three years. The other officers of the Society shall retire annually but shall be eligible for re-election, the Deputy President shall be eligible only once for re-election, and the President shall not be eligible for re-election until after the lapse of a further period of six years. Every President shall at the termination of his period of office become a Vice-President for a period of six years. Co-opted members of the Council shall cease to hold office at the Annual General Meeting of the year of their election.

23. Every year two Ordinary Members of the Council, having served for three years, shall retire and shall be ineligible for re-election until after the lapse of one year.

24. Names of Members to serve on the Council may be suggested by the Council or by any Member by sending the proposed names to the General Secretary at least two weeks before the Annual General Meeting.

24 a. Nominations of representatives of the Sections and the Branches shall be sent to the General Secretary at least two weeks before the Annual General Meeting.

25. Nominations for the election of Officers shall be handed in to the General Secretary before the Meeting preceding the Annual General Meeting and be announced at the Meeting preceding the Annual General Meeting.

26. One full week before the Annual General Meeting, the General Secretary shall send to each Member a balloting list containing: (1) The names of the Council; (2) The number of the attendance of members of the Council at the Council Meetings held during the year; (3) The names of those proposed by the Council and Committees or by Ordinary Members to fill the vacancies.

27. The Society may fill all the vacancies among the officers that may occur between two Annual General Meetings by ballot, in accordance with Rule 15, at the next General Meeting, due notice having been given. Similar vacancies among the Ordinary Members of the Council shall be filled by co-option by the Council.

28. In the absence of an ex-officio Chairman (in accordance with the conditions laid down in Rule 42) the Council shall elect one of its Members to act as Chairman of Council. A quorum at a Council Meeting shall consist of five. If there be no quorum, the Members attending shall have power to transact business, subject to the approval of absent Members, to whom the resolutions adopted shall be forwarded.

29. The first Meeting of each Special Section shall be convened by the General Secretary of the Society. At this Meeting the Section shall appoint its Chairman, Secretary and Sectional Committee.

30. The Chairman and Secretary of a Section shall retire annually but shall be eligible for re-election.

31. The size of a Sectional Committee shall be determined by the Section. A quorum at a Meeting of a Sectional Committee shall be as that Committee shall decide.

32. When Sections have been formed, Rules 12, 13, 14, 15, 21, 24, 25, 26, 27, 28, 40, 41, 42, 43, 44 shall (*mutatis mutandis* and unless expressly stated to the contrary) apply respectively to the Committees, Chairmen and Secretaries appointed by these Sections.

33. The Society shall not make any dividend, gift, division or bonus in money unto or between any of its Members.

SUBSCRIPTIONS AND FINANCE

34. Each Ordinary Member shall pay an annual subscription of one guinea, which shall entitle him to receive a copy of *The British Journal of Psychology*, with the exception of such additional portions of the *Journal* as are devoted to special branches or aspects of Psychology and are published separately.

35. Ordinary Members who are members of all Sections shall pay an inclusive annual subscription of £3. 2s.

36. In addition to the Ordinary Membership Subscription Members shall pay an additional fee for every Section which they join. This additional fee shall entitle Members of any Section to receive such additional and separately published portions of *The British Journal of Psychology* as may be devoted to the special branch or aspect of Psychology studied by the Section. The amount of such additional fee shall be fixed by the Committee of the Section concerned in consultation with the Treasurer, and any resolution passed by the Sectional Committee with reference to such fee shall, before coming into force, receive the approval of the Council.

37. Sectional Associates shall pay a subscription the amount of which shall be determined by the Sectional Committee in consultation with the Treasurer of the Society.

38. The subscription is payable in advance and becomes due on January 1st. *The British Journal of Psychology* shall be sent only to Members whose subscriptions have been duly paid.

39. The Council shall grant to each Section permission to incur such expenses as may be necessary for the work of the Section; the approximate amount of such expenses to be determined by the Council.

MEETINGS

40. The Annual General Meeting shall be held in December, when the General Secretary's Report and the Treasurer's Statement of Accounts shall be laid before the Society.

41. The General Meetings of the Society shall be at such times and places as the Council shall decide, but in each year there shall be at least four meetings.

42. The President, Deputy President, or failing them a Vice-President, shall, whenever possible, act as Chairman at the Meetings of the Council and of the Society. In the absence of the President, Deputy President, or a Vice-President, some other Member shall, on the motion of the General Secretary or his substitute, be elected to act as Chairman.

43. The Secretary shall send to all Members of the Society notices of each Meeting at least seven days beforehand, and of the business to be transacted at the Meeting.

44. An Extraordinary General Meeting may be summoned at any time by the Council, or by the General Secretary on the written request of twenty Members; at least fourteen days' notice of such Meeting shall be sent to each Member of the Society.

VISITORS

45. Each Member shall have the privilege of introducing two visitors at any Meeting. The Council or a Sectional Committee shall however have the power of closing any Meeting or Meetings to visitors, provided notice to this effect be given on the notice convening such Meeting or Meetings. The names of visitors together with the names of Members introducing them shall be entered in a book provided for that purpose. Any Member of the Society attending a Meeting of a Section to which he does not belong must be introduced by a Member of that Section.

BRANCHES

46. The Council shall be empowered to sanction and dissolve the establishment of Branches of the Society throughout the British Empire. Members of such Branches shall pay the same subscription as that of Ordinary Members direct to the Treasurer of the Society and be entitled to the same rights and subject to the same rules as Ordinary Members. The Council shall be empowered to refund to the Treasurer of any Branch such proportion of Branch Members' subscriptions as the Council may determine to meet local expenses.

46 *a*. Any Rules made by Branches of the Society shall be first submitted to the Council for their approval.

46 *b*. The minimum number of Members necessary to form a Branch shall be 10.

46 *c*. Branches shall be required to present a report to each Annual General Meeting.

46 *d*. Branches shall have the right of electing Branch Associates under conditions similar to those governing the election of Sectional Associates.

RULES

47. The Rules of the Society may be altered at the Annual General Meeting, after notice of alteration has been given at a previous Ordinary General Meeting, or they may be altered at an Extraordinary General Meeting summoned for that purpose. Such Extraordinary General Meeting shall be held in London, and the purpose for which it is called shall be expressly stated in the notice convening the Meeting. No other business shall be transacted thereat.

BRITISH PSYCHOLOGICAL SOCIETY

LIBRARY RULES

1. The Library of the Society shall ordinarily be open for the use of members from 10 a.m. to 5 p.m. (Saturdays, 10 a.m. to 1 p.m.).
2. Persons other than members of the Society may use the Library only on obtaining permission from the Hon. Librarian. Such permission shall as a rule be granted only to scientists or research students who are known personally or by reputation to the Hon. Librarian, or who are introduced to him by a member of the Society. Such persons must in all cases have declared their willingness to conform to the rules regulating the use of the library by members.
3. Members must on each visit to the library sign the book provided for that purpose.
4. No book may be marked, annotated, mutilated, or defaced in any way.
5. A member shall be responsible to the Society for any loss or damage occurring to a book while in his use or possession.
6. No books may be removed or borrowed from the library.

LIST OF PERIODICALS AND SERIAL PUBLICATIONS CONTAINED IN THE LIBRARY (1937)

* Denotes publications from year given up to date.

*Acta Psychologica. 1935-

*Action et Pensée (Bulletin de la Société Internationale de Psychagogie et de Psychothérapie). 1925-

American Journal of Insanity. 1903-1915.

*American Journal of Psychiatry. 1921-

*American Journal of Psychology. 1888-

*Annales médico-psychologiques. 1921-

*l'Année psychologique. 1909-

*Archiv für die gesamte Psychologie. 1903-

Archiv für Gewerbepathologie und Gewerbehygiene. 1930-1932.

Archiv für Kriminologie. 1922-1930.

*Archives de psychologie. 1902-

*Archives of Neurology and Psychiatry. 1919-

*Archives of Psychology. 1907-

*Archivio generale di neurologia e Psichiatria. 1921-

*Australasian Journal of Psychology and Philosophy. 1923-

Berichte über den 1-9 Kongress für experimentelle Psychologie. 1904-1924.

Brain. 1914-1921.

- *British Journal of Educational Psychology. 1931–
- *British Journal of Medical Psychology. 1920–
- *British Journal of Psychology (General Section). 1904–
- *British Journal of Psychology Monograph Supplements. No. 1–
Canadian Journal of Mental Hygiene. 1919–1922.
- *Character and Personality. 1932–
Columbia University Contributions to Education. Selected numbers.
- *Comparative Psychology Monographs. 1922–
Die Experimentelle Pädagogik. 1905–1910.
- *Educational Research Bulletin. 1928–
- *L'Encéphale. 1927–
- *Eugenics Review. 1922–
Fortschritte der Psychologie. 1913–1922.
Forum of Education. 1923–1930. (Now incorporated with the British Journal of
Educational Psychology.)
- *Genetic Psychology Monographs. 1927–
Grenzfragen des Nerven- und Seelenlebens. 1900–1914, 1917–1927.
- *(The) Human factor. (Journal of the N.I.I.P.) 1932–
- *L'Hygiène Mentale. 1927–
- *Imago. 1913–
- *Industrial Health Research Board Reports. No. 1–
Industrial Psychology. 1926–1928.
- *Industrielle Psychotechnik. 1924–
- *International Journal of Psycho-Analysis. 1920–
- *International Zeitschrift für Individualpsychologie. 1924–
- *Internationale Zeitschrift für Psychoanalyse. 1913–
Journal de Neurologie et Psychiatrie. 1927–1929.
- *Journal de Psychologie. 1904–
Journal für Psychologie und Neurologie. 1902–1934.
- *Journal of Abnormal and Social Psychology. 1907–
Journal of Animal Behavior. 1911–1917.
- *Journal of Applied Psychology. 1917–
Journal of Comparative Neurology and Psychology. 1907–1910.
- *Journal of Comparative Psychology. 1921–
- *Journal of Educational Psychology. 1910–
- *Journal of Educational Research. 1920–
Journal of Experimental Pedagogy. 1911–1921.
- *Journal of Experimental Psychology. 1916–
- *Journal of General Psychology. 1928–
- *Journal of Genetic Psychology (formerly Pedagogical Seminary). 1891–
- *Journal of Juvenile Research. 1928–
- *Journal of Mental Science. 1921–
- *Journal of Nervous and Mental Disease. 1926–
- *Journal of Neurology and Psychopathology. 1920–
- *Journal of Philosophical Studies (now Philosophy). 1926–
- *Journal of Philosophy. 1911–
Journal of Philosophy, Psychology and Scientific Methods. 1908–1914.

- *Journal of Social Psychology. 1930-
- *Journal of the National Institute of Industrial Psychology (now The Human Factor). 1924-
- *Journal of the Royal Anthropological Institute. 1909-
- *Kwartalnik Psychologiczny. 1930-
Man. 1906-1928.
- *Mind. 1915-
Nervous and Mental Disease Monograph Series. (Selected numbers.)
- *Neue Psychologische Studien. (München.) 1923-
- *Pedagogical Seminary and Journal of Genetic Psychology (now Journal of Genetic Psychology). 1891-
- *Personnel Journal. 1927-
- *Philosophical Review. 1915-
Philosophische Studien (Wundt). 1883-1902.
- *Philosophy (formerly Journal of Philosophical Studies). 1926-
Praktische Psychologie. 1919-1922.
Proceedings of the Aristotelian Society. 1916-1923.
- *Proceedings of the National Academy of Science. 1929-
- *Proceedings of the Society for Psychical Research. 1910-
- *Psyche. 1921-
- *Psychoanalytic Quarterly. 1932-
- *Psychoanalytic Review. 1913-
die Psychoanalytische Bewegung. 1929-1933.
- *Psychological Abstracts. 1927-
- *Psychological Bulletin. 1904-
Psychological Clinic. 1908-1935.
Psychological Register. 1929, 1932.
- *Psychological Review. 1894-
Psychological Review Index. 1894-1935. (Now incorporated in Psychological Abstracts.)
- *Psychological Review Monographs. No. 1-
Psychologische Arbeiten (ed. Kräpelin). 1895-1928.
- *Psychologische Forschung. 1922-
Psychologische Studien (Wundt). 1906-1917. (*See* Neue Psychologische Studien.)
- *Psychometrika. 1936-
- *Revue Française de Psychanalyse. 1927-
- *Rivista di Neurologia. 1928-
- *Rivista di Psicologia. 1905-
Science Progress. 1917-1933.
Scientia. 1926-1935.
Training School Bulletin. 1929-1931.
- *le Travail Humain. 1933-
- *University of California Publications. 1916-
Yale Psychological Laboratory Studies. 1892-1902.
- *Zeitschrift für angewandte Psychologie. 1908-
Zeitschrift für angewandte Psychologie. Beihefte.
Zeitschrift für Neurologie und Psychiatrie. 1910-1914.

*Zeitschrift für pädagogische Psychologie. 1899–

*Zeitschrift für psychoanalytische Pädagogik. 1926–

*Zeitschrift für Psychologie. 1890–

Zeitschrift für Psychotherapie und medizinische Psychologie. 1909–1915.

*Zeitschrift für Sinnesphysiologie. 1908–

In addition to the above, the Library contains a few numbers only of the following publications:

Arbeiten aus dem Psychologischen Institut der Universität München.

Archivos Brasileiros de Hygiene Mental.

Bulletin de l'Institut Général Psychologique.

Indian Journal of Psychology.

Journal of the Department of Letters, University of Calcutta.

Mental Measurement Monographs.

Pädagogisch-psychologische Arbeiten (aus dem Institute des Leipziger Lehrervereins).

Scandinavian Scientific Review.

Smithsonian Institution Publications.

Tohoku Psychologica Folia.

Travaux Psychologiques de l'Université Jean Casimir de Lwów, Poland.

THE PERCEPTION OF DISTANCE

BY M. D. VERNON

*From the Cambridge Psychological Laboratory*Part I. *An introductory consideration of the subject* (pp. 1-11).(1) *The phenomenal experiences involved* (pp. 1-4).(2) *The fundamental factors operating* (pp. 4-11).

PART I. AN INTRODUCTORY CONSIDERATION OF THE SUBJECT

(1) *The phenomenal experiences involved*

THE subject of distance perception has been the battle-ground of theorists and the despair of experimentalists for many a long year. But it may be that this confusion has resulted to some extent from a faulty approach to the subject. One outstanding criticism of this approach relates to the general failure to study and analyse the phenomenal experiences which are connected with distance perception. These include a number of perceptual and ideational experiences which frequently overlap and closely shade into one another. But for all that it behoves us to distinguish them as far as possible, since the conditions under which each one characteristically occurs are by no means identical. Consequently, if we are to study the factors upon which distance perception is based, we must decide which type of experience we wish to consider.

Numerous authors have distinguished between immediate and secondary perceptions and measurements of distance, and this distinction is a fundamental one, though it is unwise to assume that either type is rigidly connected with the employment of any given set of perceptual or ideational data. Among the immediate experiences may be distinguished:

(1) A direct perception of spatial distance, which tells us that the surrounding world is not flat but has a three-dimensional structure, and that its parts are not all at equal distances but are arranged in gradually receding planes. This experience is independent of any deliberations as to the nature of these parts or of any careful comparison of their relative distances. It is closely related to:

(2) A direct perception of depth or relief, which is dependent upon an appreciation of the interrelationships of solid objects and surfaces in

relief, particularly of those in contiguity with each other. It may be that (1) is developed from (2); it is certainly closely connected to it in experience.

(3) Qualitative estimates of distance made loosely in terms of 'near' or 'far' in absolute distance estimation, or of 'nearer' or 'farther' in relative distance comparison, can probably be formed directly and immediately, and may thus also be termed immediate processes.

All these experiences are so quickly and easily obtained and the processes underlying them are so little conscious that we are tempted to assign them to some innate capacity for perceiving spatial relations. However that may be, we immediately notice a difference when any kind of careful and quantitative estimation of absolute or relative distance is required. The experience of immediacy disappears, and there appears instead a careful study of all the perceptual data, followed frequently by comparison with relevant ideational data, and a final assessment leading to a deliberately made response. Such an experience may properly be called secondary; in it, direct perception passes over into delayed judgement.

The distinction between these two types of experience is by no means confined to distance perception, but can be recognized in almost any perceptual situation, particularly under experimental conditions. Madison Bentley (3) has termed them 'perception' and 'inspection' respectively. 'Inspection' allied with close scrutiny is liable to appear whenever the conditions of observation are difficult, with unfamiliar or incomprehensible perceptual objects or situations, unfavourable subjective conditions, and so on. Hunt (20) has found it to be particularly noticeable in the early stages of psycho-physical experiments in which the observer is required to distinguish barely perceptible stimulus differences; with practice, the amount of inspection decreases, and it is replaced by immediate perception. This is a fact of great importance, for inspection is usually accompanied by strong feelings of strain, tension and even of apprehensiveness, and is apt, particularly in psycho-physical experiments, to produce objective results which are less certain and accurate than those based on immediate perception. Other workers have confirmed the unfortunate effects of this strain upon accurate perception and judgement, and have found that reliable results can only be obtained once the observer has learned to make easy, rapid and semi-automatic perceptual estimates (see George (13)).

But unfortunately the conditions of certain experiments are such that this is not always possible. We have already mentioned that as soon as quantitative estimation of distance is required, judgement as

well as perception begins to function. It seems quite possible that this is characteristic of all quantitative estimation of sensory and perceptual qualities. The observer can base a response made in terms of either-or (same-different, darker-lighter, nearer-farther) upon an immediate perception of difference—the ‘step-wise phenomenon’ of Koffka⁽²⁵⁾. But if quantitative estimation or comparison is required, as for instance in the ‘equal-appearing intervals’ experiment, or in the absolute estimation of distance (except quite vaguely, as ‘near’ or ‘far’), the observer is forced, in order to judge the magnitudes of the particular qualities involved, to consider them as separately measurable entities, not as a single integrated configurational experience. This involves a judgement—the entity is judged to equal some concept of magnitude—which is essentially ideational and cannot be directly perceived.

But even when comparisons rather than quantitative estimates are required, it seems probable that, under the highly artificial and difficult conditions prevailing in many experiments on perception of distance, inspection would be very likely to supersede perception. The responses to be made are based upon perception of very small differences in isolated sensory qualities or details of formal structure which the observer is not normally accustomed to observe or estimate at all closely. This supposition is supported by three observations:

(1) Previous experimenters have reported the uncertainty, hesitation and inconsistency of the subjects’ responses, and we have seen that these are characteristic of the process of inspection, as against that of immediate estimation. This is particularly the case in the work of Wundt⁽⁴¹⁾, Hillebrand⁽¹⁹⁾, Dixon⁽⁸⁾, Arrer⁽¹⁾ and Baird⁽²⁾ on estimation of distance in monocular vision.

(2) When conditions such as those obtaining in many previous experimental situations (for instance, in monocular vision) were repeated in the experiments described in this paper, some of the subjects reported that although they could estimate and equate the relative distances of two objects because they had been told these objects were not, to start with, equidistant, yet in fact the objects did not really ‘look’ as if they were at different distances, because the perceptual field ‘looked’ quite ‘flat’.

(3) Certain data such as deliberate comparisons of apparent size, brightness, etc., which appear to function prominently in some experimental situations, are dependent in part on ideational factors involving previously acquired knowledge. The deliberate use of such ideational data can hardly occur except in secondary judgement processes.

Thus we may suspect, and our suspicions are amply confirmed by the results of the experiments described in Part II, that in much of the previous experimental work immediate perception of distance has been too frequently superseded by secondary processes of inspection and deliberate judgement. We do not wish to deny that such processes may play an important, possibly an essential, part in producing the experiences connected with distance perception. But we confidently assert that it is only a part, and not the major part. If we are to gain any real insight into the essential experiences of distance perception and the processes underlying them, we must not exclude by our experimental procedure the primary and immediate perceptions of distance and depth.

(2) *The fundamental factors operating*

Bearing this qualification in mind, we may now study some of the earlier experimental work on the estimation of distance, which was designed to investigate the outstanding factors operating. Experiments to show the accuracy of estimation of distance in monocular and binocular vision were carried out firstly by Wundt⁽⁴¹⁾, and later by Hillebrand⁽⁴⁹⁾, Dixon⁽⁸⁾, Arrer⁽¹⁾, and Baird⁽²⁾. The main aim of these experiments was similar: to simplify the conditions of experiment as far as possible, and thus to isolate a single important set of perceptual data by eliminating or rendering inoperative all other possible data. In the experiments of Wundt⁽⁴¹⁾ the test objects were black threads viewed through a tube against a white ground, all surrounding objects thus being obscured from view. The absolute distances of a single thread were estimated by the observer; he was also required to state whether the thread, when moved, appeared to approach him or recede from him. Relative localization with regard to a second thread was also measured. It was found that although the observer was unable to form any definite estimates of absolute distance in monocular vision, he could perceive changes of distance within certain limits, and could perform relative localization rather inaccurately. In binocular vision, absolute localization was less indefinite, though still highly inaccurate; differences of distance were estimated much more accurately than in monocular vision. Wundt concluded from this that the kinaesthetic sensations of changes of muscular tension involved in convergence movements were the chief factors involved in producing the much greater accuracy of binocular relative localization. Sensations of accommodatory tensions were utilized in monocular vision, and also to some extent the change of apparent size of the thread as it moved.

In order to eliminate the size factor, Hillebrand⁽¹⁹⁾ used as test object a black screen, only the edge of which was visible, against a white ground. This screen he either moved gradually towards or away from the subject, who used monocular vision alone, or replaced abruptly by another screen at a different distance. He found that in the first case the observers were unable to perceive the direction of motion; and that even relative localization, in the second case, was very uncertain. From this he concluded that sensations of accommodation were never spontaneously employed in distance estimation; but that in relative localization the observer perceived that the retinal image of the second screen was blurred, and deliberately adjusted his accommodation to and fro until it appeared clear. These adjustments gave him some idea of the relative distances of the objects, but were of course useless for absolute localization. That the use of accommodation sensations was conscious and deliberate was, however, denied by Dixon⁽⁸⁾, who considered that they provided an indirect but fairly accurate basis for relative localization.

On the basis of this and another experiment (which we shall describe subsequently, in Part II), Hillebrand concluded that the principal factor in monocular vision was the apparent size factor—whenever this was allowed to operate. Arrer⁽¹⁾, however, criticized Hillebrand's apparatus on the ground that the edge of the screen was too indefinite an object for any accurate fixation and focusing of the eye. He used an apparatus similar to Wundt's, and obtained monocular liminal values for difference of depth not very much greater than the binocular ones. He concluded that sensations of convergence played the principal part both in monocular and binocular vision; in the former case, reflex convergent movements of both eyes occurred in response to adjustments of accommodation in the unclosed eye. Baird⁽²⁾ also controverted Hillebrand's conclusions, though on different grounds. He used an apparatus similar to Hillebrand's, but in the monocular vision of the moving screen the movement started after the subject had begun looking at it. In consequence, the subject was able to make immediate judgements, without any 'conscious impulse of will', as to the direction of movement; and on the whole these judgements were fairly consistent. Baird concluded that in Hillebrand's experiments accommodation was not functioning normally, whereas in his own, where it was normal, it provided an adequate basis for monocular distance estimation. It may be pointed out, however, that as the subject usually perceived that there was movement before he could see which way the screen was moving, it is possible that monocular sensations of disparity resulting from movement were of assistance.

Baird also carried out experiments with binocular vision and found that they gave more accurate results than any of those with monocular vision, from which he concluded that convergence sensations were the principal factors in binocular vision. The controversy as to the relative importance of convergence and accommodation was taken up again much later by Swenson⁽³⁷⁾, who designed an apparatus which he claimed directly demonstrated the relative importance of the two factors in binocular vision. The test objects were two illuminated discs, situated at the end of tubes, which gave a single fused binocular image of constant retinal size; they were viewed through rotating mirrors in such a way that either (1) convergence was changed without change of distance, and thus of accommodation, or (2) distance, and therefore accommodation, was altered without change of convergence. The subject estimated the absolute distance of the fused image by equating to it the distance of a pointer, viewed directly, which he could slide along a metal rod. When the apparatus was adjusted in such a manner that convergence and accommodation corresponded to different object distances, it was found that the object was located at an intermediate position, but at one much nearer to the position as indicated by convergence than to that indicated by accommodation. From this he concluded that, for object distances of 25-40 cm., convergence was a factor of much greater importance in binocular vision than was accommodation, although the latter did play some part.

From all these results, then, it might be concluded that the main factor in producing binocular relative distance perception and estimation was constituted by the kinaesthetic sensations of change of convergence tension. We shall now criticize this conclusion in detail, and return later to the importance of accommodation. In the first place, under the conditions of these experiments, the observer was prevented from using much of the normally employed perceptual data, and was confined to the one or two remaining sets. Thus he was forced to utilize these in order to make any sort of estimate. Whether or not he would make equal use of them under normal conditions, we have no means of determining. We cannot assume that in normal vision these particular types of data have anything like the same importance that they possess in isolation. Moreover, such estimates, particularly the monocular ones, may easily become a matter of secondary judgement, not of perception at all (cf. Hillebrand's conclusion as to the functions of accommodation).

Secondly, with regard to the fancied importance of convergence sensations, it must be remembered that changes of convergence would

not arise except in response to stimulation from some change in the retinal sensory pattern. This might have been the blurring which occurred whenever accommodation was not correctly adjusted. This factor would also of course operate in monocular vision. But since binocular distance estimates were much more accurate than monocular ones, some other factor must also have been operative with the former. Some aspect or quality of the pattern of binocular stimulation must then have varied with distance in the absolute localization experiments; and must have differed for the two stimulus objects in the relative localization experiments. A possible explanation might have been that as the distance of the object altered, the retinal images at the two eyes became too unlike to fuse into a single binocular image, and double images were produced. But double images were rarely reported in these experiments. Moreover, the point at which fusion breaks down is by no means an invariable function of the degree of unlikeness of the two retinal images as measured by the difference of visual angle subtended at the eye by the two objects, or the same object at different distances. Hence there is no constant relationship between change of distance and occurrence of diplopia. Glanville⁽¹⁴⁾ showed that whether or not diplopia will occur at non-corresponding retinal points is a matter of great uncertainty with objects the precise spatial location of which is not indicated by other cues. Sander⁽³⁵⁾ found that if one of a pair of stereoscopic slides was rotated about an axis, the angle at which fusion broke down increased with increasing structural complexity of the slides. Thus diplopia occurred most easily with a pair of horizontal lines, but least easily with a pair of stereoscopic photographs of buildings. Koffka⁽²⁶⁾, again, presented to each eye a drawing of a pair of equidistant horizontal parallel lines; one member of each pair was dotted in such a way that the dots in the right eye field coincided with the spaces between the dots in the left eye field, when the two fields were fused in the stereoscope. Thus the combined dotted line appeared as a full line, and this fusion was maintained when one of the dotted lines was moved sideways—that is to say, when the spatial distance of the dotted line from the other full line was greater in one retinal field than in the other. Fusion was thus a function of the formal relationship of the two dotted lines, rather than of the similarity of the two retinal images. Hence we must conclude that no accurate estimation of depth or distance could be made from observing the point at which fusion breaks down.

But parts of the field at different distances from the eye, subtending different angles at the retina, produce impressions which fall on non-corresponding points of the retina—that is to say, at points at different

retinal distances from the fovea. Thus there is an unlikeness or disparity of the retinal images, based upon the fact that the spatial relationship between the fixation point and the other retinal points stimulated is different for the two eyes; although the images still fuse to form a single binocular image. This impression of disparity might either produce reflex convergent movements, the kinaesthetic sensations from which were interpreted by the observer as a change of distance; or might be directly experienced as an immediate perception of depth or change of distance. All the evidence goes to support the latter hypothesis.

In the first place, photography of the moving eye by Judd⁽²²⁾ showed that the actual movements executed by the eyes in converging to a given position were extremely erratic and variable. Large convergent movements were followed by a series of corrective jerks, and even then the final fixation position was often asymmetrical for the two eyes, and was different upon successive occasions. Later, Clark⁽⁷⁾ showed that impressions of two stereoscopic slides falling on any point within $1-1.5^\circ$ of the fovea produced a single fused binocular image. Clearly then the direction and extent of the movements, and hence of the corresponding changes in convergence tension, are too uncertain and variable to provide any accurate basis for distance estimation. The same criticism applies probably to the utilization of sensations of accommodation; but we shall discuss subsequently the part played by the retinal impressions from the unaccommodated eye (see Part II).

In the second place, experiments with the stereoscope seem to show that at any rate perception of depth—that is to say, of differing distances in the field—results directly and immediately from impressions of disparity under the very simplest conditions. Thus stereoscopic slides each of a pair of dots at different distances from each other will be fused to appear as a single pair of dots, one dot nearer than the other to the observer; the same effect was obtained in Koffka's experiment⁽²⁶⁾ with the fused dotted line. Again, in the classic experiment of Dove⁽¹⁰⁾, stereoscopic relief was perceived when the fields were momentarily illuminated by an electric spark; this time was too short to allow any convergent movements to occur. The same effect was shown by Hering's fall experiment⁽¹⁸⁾. Small white beads were allowed to fall across an aperture in front of a dark background. The subject was able accurately to localize their distance relative to that of a stationary bead when using binocular vision, though he could not do so in monocular vision. Here again the time was too short to allow convergent movements, though it is possible that the relative size factor might have been operative.

Thus it seems probable that immediate perception of non-equidistance in the visual field is essentially connected with the unlikeness or non-correspondence in the retinal images, falling short of that which gives rise to actual double images. This unlikeness is seen directly and immediately as non-equidistance. Thus the characteristic 'flatness' of a field perceived monocularly is due to the lack of disparity. The perception of disparity can reach great degrees of refinement; in certain stereoscopic instruments a difference of visual angle subtended by two objects of as little as 4 sec. can be perceived and adjusted for.

A similar effect of monocular depth may be obtained by projecting two stereoscopic images of an object in succession on to the same area of the single retina; the same effect occurs when the head is moved rapidly to and fro. Thus Bourdon⁽⁵⁾ found it was possible accurately to determine the relative position of two bright points of light in a dark room if the head were moved, but not otherwise. Watt⁽³⁹⁾ showed that monocular depth perception could be perceived by projecting two stereoscopic slides alternately on to a screen, at intervals of 0.2 sec.; or by moving the eye relatively to the projection of a single slide, or the projection relatively to the eye. Thus apparently there is no inherent difference between binocular and monocular perception of disparity; depth vision is simply a function of the integration of the two disparate images, whether separated temporally (as in the monocular experiments) or spatially (as in the binocular experiments).

But we must not assume that disparity can alone determine and account for all the phenomena of depth and distance perception and estimation with which we are familiar. Four essential qualifications must be made:

(1) Disparity is perceived directly as non-equidistance; but which part of the field is nearer and which farther cannot always be perceived immediately from disparity alone. Hering⁽¹⁷⁾, it is true, stated that parts of the field which stimulate the nasal halves of the retinas appear as farther than the fixation point, and parts which stimulate the temporal halves as nearer. The experiments of Wundt and the others which we have described also showed that a fair accuracy of estimation of 'nearer' and 'farther' was possible on the basis of disparity alone. But we have already stated that under very simplified conditions the observer may be forced to make estimates of this kind in a manner quite foreign to him under normal conditions. Numerous experiments have shown that if the conditions are more complicated and other types of perceptual data are included besides those relating to disparity, estimates of 'nearer' and

'farther' may depend on the former rather than on the latter. Thus if stereoscopic slides of landscape scenes are presented in reversed relationship, the perspective does not as a rule appear reversed (Eaton⁽¹¹⁾); the nearer and farther relationship is determined by the formal relationships of the figures, not by their disparity relationships. These results are confirmed by the experiments of Kopferman⁽²⁸⁾ and by the experimental results described below (see Part II).

(2) There is no fixed relationship between degree of disparity between two objects and amount of difference of visual angle subtended—that is, of their distance from each other. Thus Poppelreuter⁽³²⁾ showed that if an arrangement of receding points was presented to the subject which produced the effect of equal intervals of depth, the points did not subtend equal differences of visual angle, but were situated at approximately equal arithmetical distances apart. Moreover, the monocular and binocular arrangements were practically the same. Thus he concluded that differences of binocular disparity were not utilized in making such arrangements. It seems probable that perspective effects and size relationships between the spaces between points were of more importance.

(3) There seems to be some evidence to show that the observer must be trained or 'set' to perceive disparity, though once he is 'set' he perceives it immediately as difference of distance. Thus Lavelle⁽³⁰⁾ denied that there was any spontaneous perception of relief in the Dove experiment; and considered that it appeared instantaneously merely by force of habit, since perception of stereoscopic relief is normally a slowly developing process, necessitating movements of 'attention' from one point of the field to another. Donders⁽⁹⁾, indeed, according to Rivers⁽³⁴⁾, found that several instantaneous illuminations might be required to perceive the relief adequately. Lastly, Karpinska⁽²³⁾ found that if the observer did not know before the exposure whether the figure exposed would be flat (i.e. the two retinal images identical) or stereoscopic, he frequently failed to perceive stereoscopic relief even in the latter case. Even when the observer is 'set' to perceive disparity, his accuracy in perceiving it may be increased with practice, as, for instance, in the use of stereoscopic instruments. Thus we may conclude that, since an observer is not obliged under normal conditions to perceive disparity isolated from all other perceptual data which indicated depth or non-equidistance, he will not always do so when it is thus isolated, unless he is practised and prepared for this type of perception. But this conclusion does not of course disprove the fact that once disparity has been perceived, it appears immediately and directly as depth, or non-equidistance.

(4) Disparity will become inappreciable with far-distant objects, when the angular difference falls below the perceptual limen. For an individual of normal visual acuity this difference is reckoned at 110 m. for objects, the retinal impressions of which fall on the macula—that is to say, can be seen simultaneously without rotation of the eyes (Eaton (11)). Beyond this distance other factors, to be described subsequently, must come into play.

Thus we may conclude that disparity is an essential factor in our normal perception and estimation of distance, but that it is usually accompanied by other types of perceptual data which assist the formation of rapid and accurate percepts of equidistance and non-equidistance, and also of quantitative estimates of amount of distance, both relative and absolute. Among these types of perceptual data the most important seem to be formal or structural characteristics connected with relative apparent size, brightness and relations to surrounding parts of the field. These we shall discuss together with the relevant experiments in the second part of this paper, which deals with the experimental treatment of the subject. (References will also be given at the end of Part II.)

(Manuscript received 15 January 1936)

A STUDY OF THE SIGNIFICANCE OF ATTITUDE IN RECOGNITION

BY O. L. ZANGWILL

From the Cambridge Psychological Laboratory

- I. *Introduction* (p. 12).
- II. *Experimental procedure* (pp. 13-14).
 - (a) *The experimental group* (p. 13).
 - (b) *The control group* (pp. 13-14).
- III. *Results* (p. 14).
- IV. *Discussion* (pp. 14-17).
- V. *Summary* (p. 17).
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I. INTRODUCTION

PROF. BARTLETT (1), p. 191) has pointed out that recognition is dependent upon the coincident arousal of two different functions, (a) a specific sensory reaction and (b) an attitude, or orientation, that cannot be ascribed to any localized physiological apparatus. He believes that for recognition to occur, that attitude which marked an original presentation must be carried over to re-presentation. The present investigation represents an endeavour to assess the significance of this attitude factor in recognizing, and may be regarded as an attempted resolution of one of the many problems suggested by Bartlett's highly significant generalization.

The method consists essentially in instigating a 'preparatory set' in the subject by means of appropriate experimental instruction. The subjects are instructed to perceive the specific experimental material in a certain specific manner, i.e. they confront the material with a well-marked attitude predetermined by such instruction. Subsequently, an attempt is made to establish a totally different 'set' through alteration of experimental instruction. A further series of experimental material is now exposed, including, however, some of the material that has already been presented. If Bartlett's hypothesis be substantially correct, this very fact of alteration in attitude resultant on modification of experimental instructions would be expected to prevent recognition of the previously exposed material.

II. EXPERIMENTAL PROCEDURE

(a) *The experimental group.* Thirty subjects co-operated in the main portion of the experiment. The material comprised two series of five ink-blots. The members of the first series had a faint representational significance in as much as they all bore a vague resemblance to animal forms; members of the second series had likewise an ill-defined resemblance to mountainous scenery. A 'critical' blot, lacking any determinate representational significance, was employed as a common member to both series. The members of the first series were exposed successively in Grindley's tachistoscope(2), each for 0.3 sec. Before exposure, the subject was asked to read the following instructions: "You will be shown a series of six cards, on each of which is a black blot on a white ground. Each card will be exposed for 0.3 sec. *Each of these blots is intended to suggest an animal.* Immediately after each exposure, please write down what the blot suggested to you, together with a rough reproduction of the figure." The 'critical' blot was exposed as fifth member of this series. Directly after the last member of the first series had been presented, the subject received another card on which were the following instructions: "You will be shown a series of six cards, on each of which is a black blot on a white ground. Each card will be exposed for 0.3 sec. *Each of these blots is intended to suggest mountains or Alpine scenes.* Immediately after each exposure, please write down what the blot suggested to you, together with a rough reproduction of the figure." The second series was now exposed, of which the sixth member was the 'critical' blot. After both series had been exposed, the subject was asked if he had noticed whether any one or more cards were common to both series, and if so, to state which cards he believed them to be.

(b) *The control group.* Ten subjects constituted the control group. The eleven cards which constituted the two series for the experimental group were shuffled up, and exposed successively as before, but this time as members of a single series. The subjects received the following instructions: "You will be shown a series of twelve cards, on each of which is a black blot on a white ground. Each card will be exposed for 0.3 sec. *Some of these blots are intended to suggest animals, others mountains or Alpine scenes.* Immediately after each exposure, please write down what the blot suggested to you, together with a rough reproduction of the figure." The 'critical' blot was again exposed on two occasions, as fourth and as twelfth member of the combined series. After the entire series had been exposed, each subject was asked if he had noticed whether any one

or more cards had been twice presented, and if so, to state which cards he believed them to be.

III. RESULTS

	Experimental group (30 subjects)	Control group (10 subjects)
	%	%
Correct recognition of critical blot	36.7	90
Non-recognition of critical blot	63.3	10

IV. DISCUSSION

The results show clearly that alteration in attitude resultant upon alteration in experimental instruction may frequently prevent recognition—at any rate, recognition of such material as was employed in this investigation. This result would certainly seem to justify Bartlett's emphasis upon the significance of the attitude factor, and to support his view that reinstatement of a sensory reaction constitutes in itself an inadequate condition for recognition. The qualitative results of this investigation enable us, however, to obtain further information with regard to the mode of operation of attitudes in perception, and to suggest possible reasons why alteration in attitude may lead to such significant consequences for recognizing.

Fig. 1 depicts copies of each pair of reproductions of the 'critical' blot from six subjects, all of whom failed to recognize the blot on its second exposure. If we can regard these reproductions as in any way criteria of perceptions, it strongly looks as though recognition were prevented on account of the totally different nature of the perception (phenomenal appearance, behavioural object) in the two cases, resultant upon the operation of two different 'preparatory sets' or attitudes.

This type of phenomenon in perception has, of course, been long known, but has usually been interpreted in terms of 'apperception'. This latter activity was, as a rule, itself referred back to various functions such as 'association', 'assimilation', or 'fusion'. Apperceptive doctrines were further very frequently of the atomistic type, and even the more enlightened contemporary varieties (e.g. that of Stout) have taken no account of more recent experimental work. The results of modern research in the sphere of perception may be said to demand a response theory.¹ Thus perception may be defined as a response of the organism to a specific pattern of peripheral stimulation. Such a theory has, however,

¹ This point is emphasized particularly by Thouless (3) in his theoretical interpretation of phenomenal regression.

still to account for the operation of past experience in such a mode as that demonstrated in the present investigation. If a response theory is to be uncompromisingly accepted, it must account for these facts as well.

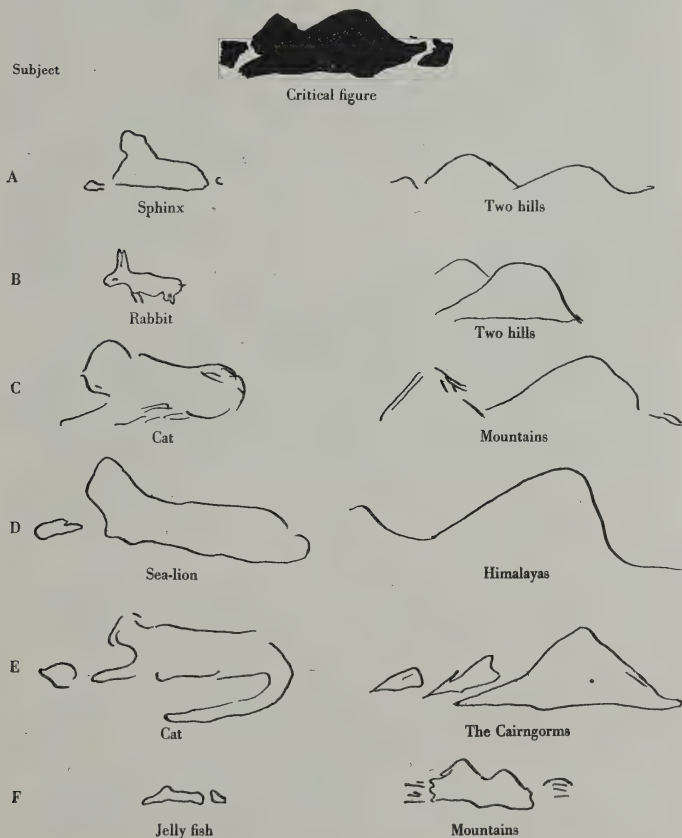


Fig. 1.

Hence, to the present writer, there seems no alternative other than to follow Bartlett in regarding both sensory pattern and psychological attitude as *co-determinants* of the perceptual response. But this hypothesis

would not seem completely adequate unless we can regard the co-determining attitude itself as a direct function of an organised body of past experience. Though not expressly stated by Bartlett, this would seem an inevitable corollary of his theory of schemata. "In recognizing", he writes, "the scheme...uses the organism...to produce a differential reaction...There is a reaction *by means of* organized psychological material" (1), p. 196, original italics). Furthermore, Bartlett's very definition of schema refers to "an active organization of past reactions, or of past experience, which must always be supposed to be operating in any well-adapted organic response" (1), p. 201). The perceptual response, far from comprising a mere fusion of discrete images with discrete sensations, must hence be regarded as a resultant both of the sensory reaction and of an organized body of past experience. But we have already defined it as a resultant of sensory reaction *and of attitude*. Perhaps the factor of attitude and the factor of schematic determination might be related in the following manner: one might consider the experimental instructions as activating a particular scheme, and this process as being directly responsible for the well-marked subjective 'set' or attitude.¹ The subsequent perceptual response is truly co-determined by attitude, but this is secondary and derivative in as much as the basic factor which must be regarded as pre-eminently operative in determining this response is the factor of a *particular schematic organization*.² It would then follow that recognition obtains only if the *same* schema, or organized body of past experience, co-determines the perceptual response both on presentation and on re-presentation.

This hypothesis, though necessarily incomplete and tentative, would account for the literal participation of organized past experience in a present perceptual response, and, furthermore, it links up directly with Bartlett's conception of perceptual 'dominance' (1), pp. 31-32).

An examination of the pairs of reproductions depicted in Fig. 1 constitutes a forcible demonstration of Bartlett's conception of the selective nature of subjective orientations, and of the dependence of the phenomenal dominance of certain features and aspects in a perceived pattern upon interest and attitude. On the basis of such an examination, it would appear justified to infer that different dominant characteristics were manifested phenomenally by the same objective figure on each of its

¹ Compare Ach's conception of sub-excitation of reproductive tendencies giving rise to the meaning of an idea of end and furnishing the origin of determining tendencies (Titchener(4)).

² An analogous attempt to relate attitudes and past experience is seen in Koffka's conception of "Gestalt Dispositions" (5), pp. 613-14).

two exposures. More boldly, it may be stated that unwitting valuation and revaluation of dominance among the characteristics of a perceived pattern is a direct function of attitude. Perhaps, moreover, the actual reason for failure to recognize may lie in this very factor of revaluation of dominance among the features of the perceived pattern.

V. SUMMARY

1. If the attitude that marked the original presentation of a given ink-blot was replaced by a different attitude on re-presentation, recognition failed to obtain in the case of 63·3 per cent of the subjects employed.

In the absence of such active counteracting of attitude, correct recognition occurred in 90 per cent of the subjects employed for the 'control' experiment.

2. Where the attitude which marked the presentation of such material is replaced by a different attitude on re-presentation, the nature of the perceptual response on the second occasion is likely to differ significantly from the nature of the original perceptual response.

Such differences concern pre-eminently:

(a) Those features and relationships which function as 'dominant' in the perceived pattern;

(b) That particular organization of past experiences which must be regarded as operative in co-determining the perceptual response.

3. Suggestions are advanced with a view to relating theoretically the operation of attitudes in perception with the factor of its schematic co-determination.

4. On the basis of such suggestions, it is concluded that recognition is likely to occur only if the perceptual response to a given pattern of peripheral stimulation be co-determined both on original and subsequent occasions by the same schema.

I am greatly indebted to Prof. Bartlett for his kindness in discussing with me many of the theoretical points arising out of this investigation.

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(Manuscript received 11 May 1936)

THE INFLUENCE OF MODERATE AND SEVERE INTOXICATION ON REMEMBERING

BY C. R. MARSHALL

- I. *Statement of the problem* (pp. 18-19).
- II. *The constancy of the mental states produced* (pp. 19-22).
 - (a) *Reaction times* (pp. 19-21).
 - (b) *An aiming test* (pp. 21-22).
- III. *Experiments on remembering: the tasks assigned* (pp. 22-23).
- IV. *Results* (pp. 23-27).
 - (a) *With moderate intoxication* (pp. 23-24).
 - (b) *With severe intoxication* (pp. 24-27).

I. STATEMENT OF THE PROBLEM

TOWARDS the end of last century the so-called 'law of dissolution' was applied to the action of general anaesthetics. Their effects, when given by inhalation, were compared to successive removal of parts of the brain from above downwards; the cerebrum appeared to be affected in the inverse order of its development, the latest acquired centres being affected first and the most primitive centres last. Thus consciousness, sensibility and motility are early annulled; reflex actions and other effects associated with the basal ganglia and spinal cord are influenced later; while the fundamental centres for respiration and circulation in the medulla oblongata are vitally affected last of all. How far such a generalization can be applied to the development of the higher cerebral functions within the realm of consciousness has not been determined. After a research on Indian hemp 40 years ago, the idea of investigating the possibility of different mental or functional levels and of the extent to which they might be influenced by special abilities and previous education occurred to me. But an opportunity of making observations on these points did not arrive until more than 20 years later. The results of the observations suggest that mental levels at which the performance of varied kinds of work is as constant as in a normal state can be induced and that broadly speaking the work which can be performed is influenced in proportion to its accepted difficulty. The present paper deals only with that part of the research concerned with remembering.

If definite mental levels exist it is obvious that the method employed to induce them must produce a change which is constant and uniform over the time of the experiment; and it must be one which exerts no specific action on the brain or indeed on any living tissue. The condition which fulfils these requirements is anoxaemia, since oxygen is a necessity for the vital activity of the cells of the body. Cells may vary in their oxygen requirements owing to varying metabolic activity, but it is improbable that any great variations occur among the different cerebral cells. Insufficiency of oxygen would therefore seem to be adapted to the end in view.

The most suitable method of inducing anoxaemia is by the inhalation of nitrogen. This inert gas when inhaled rapidly produces unconsciousness. It would have been possible to have used nitrogen followed by nitrogen-oxygen mixtures to produce the cerebral condition desired, but the method is unpleasant and in my case not free from danger. A few experiments of this kind were made, but the results were not distinctly different from those obtained by the inhalation of other inactive gases in experiments less unpleasant. The gases chiefly used were nitrous oxide, ethylene and acetylene; but ethyl chloride and methyl ether were also employed. These gases are absorbed and excreted unchanged, and there is no evidence that they specifically affect any cerebral centres or other tissue cells. Moreover, they can be given by inhalation with sufficient oxygen to sustain life and by proper administration of prepared mixtures in large gas bags—the largest used accommodated over 1000 litres—a constant partial pressure of the gas can be maintained in the pulmonary alveoli and thereby a constant concentration obtained in the blood and tissue fluids. This is the only method adapted for the study of the degrees of intoxication dealt with in this paper. The experiments were conducted on myself, 13–15 years ago, when I was in the early fifties. There were no fear or anxiety and no local or reflex effects; and, what is most important, I endeavoured to maintain my awareness and do my best in every state of intoxication induced.

II. THE CONSTANCY OF THE MENTAL STATES PRODUCED

(a) *Reaction times*

The constancy of the mental states produced is best shown by measurable reactions mechanically recorded. For this purpose reflex times are most suitable and many thousands of these were taken at varying degrees of intoxication during the research. As uniformity of

performance at different levels of intoxication is the only point to be stressed here, two examples of the results obtained at different levels induced in one continuous experiment—one of increasing, the other of decreasing levels—will suffice. The data are given in Tables I and II. Both are instances of intoxication produced by nitrous oxide. The reaction times were taken at frequent (usually more than thirty to the

Table I. *Simple reaction times under increasing concentrations of nitrous oxide*

Inhalation of	Time after inhalation commenced min.	Simple reaction time sec.
Normal air	—	0.23 $\begin{smallmatrix} +0.07 \\ -0.03 \end{smallmatrix}$
10 % N_2O	2-3	0.26 $\begin{smallmatrix} +0.12 \\ -0.04 \end{smallmatrix}$
	7-8	0.27 $\begin{smallmatrix} +0.07 \\ -0.05 \end{smallmatrix}$
	10-11	0.27 $\begin{smallmatrix} +0.13 \\ -0.05 \end{smallmatrix}$
	13	—
Changed to 30 % N_2O at	15-16	0.30 $\begin{smallmatrix} +0.06 \\ -0.04 \end{smallmatrix}$
	20-21	0.30 $\begin{smallmatrix} +0.10 \\ -0.06 \end{smallmatrix}$
	24	—
Changed to 50 % N_2O at	25½-26½	0.33 $\begin{smallmatrix} +0.09 \\ -0.07 \end{smallmatrix}$
	30-31	0.37 $\begin{smallmatrix} +0.07 \\ -0.11 \end{smallmatrix}$
	35-36	0.38 $\begin{smallmatrix} +0.05 \\ -0.07 \end{smallmatrix}$
	47-48	0.37 $\begin{smallmatrix} +0.13 \\ -0.07 \end{smallmatrix}$

minute) but irregular intervals. A succession of responses taken at the beginning of the research were—0.20, 0.22, 0.22, 0.24, 0.22, 0.20, 0.22, 0.20, 0.20, 0.22, 0.22, 0.24, 0.22, 0.24, 0.24, 0.22, 0.20, 0.22, 0.20, 0.22, 0.20, *aver.* 0.22 ± 0.02 sec. The normal given in the first table was that taken just before this particular experiment.

The standard of performance was not exactly the same at each intoxication level. For the same concentration of gas it was invariably found that a better performance was obtained for a given concentration if a larger concentration had previously been administered. In Table II the final performance under 20 per cent nitrous oxide is seen to be slightly better than the performance with an initial concentration of 10 per cent as shown in Table I. In an experiment in which the administration was the reverse of that in Table I, the final responses under

10 per cent nitrous oxide were $0.23 + 0.03$ sec., while in a severer type of experiment where 70 per cent nitrous oxide was breathed to unconsciousness, followed by 50 per cent and then 30 per cent, the reaction times after a balance had been established with this strength remained for the 9 min. they were taken at $0.27 + 0.11$ sec.
 $- 0.07$
 $- 0.05$

Table II. *Simple reaction times under diminishing concentrations of nitrous oxide*

Inhalation of	Time after inhalation commenced min.	Simple reaction time sec.
60 % N ₂ O	1-2	$0.25 + 0.17$ $- 0.05$
	2-3	$0.32 + 0.24$ $- 0.10$
	5-6	$0.46 + 0.20$ $- 0.26$
Changed to 40 % N ₂ O at	6	—
	$6\frac{1}{2}$ -7	$0.48 + 0.55$ $- 0.20$
	$12\frac{1}{2}$ -14	$0.40 + 0.44$ $- 0.14$
Changed to 20 % N ₂ O at	14	—
	18-20	$0.28 + 0.08$ $- 0.04$
	42-44	$0.26 + 0.04$ $- 0.04$

Uniformity of response was also found in anoxaemia. After the inhalation of nitrogen to unconsciousness and its replacement by 10 per cent oxygen, which caused cyanosis to disappear in 5 min., the responses for reaction times with choice between red and green lights was for the periods 6-7 min., $0.27 + 0.05$ sec.; 10-11 min., $0.27 + 0.03$ sec.; 21-22 min.,
 $- 0.03$
 $- 0.03$

$0.27 + 0.05$ sec., which are the same as normal $0.27 + 0.09$ sec. But in a second experiment in which the inhalation of pure nitrogen was replaced by 7 per cent oxygen, and in which cyanosis was noted 10 min. after the change, the reaction times were increased, namely: 10-12 min.,
 $0.31 + 0.09$ sec.; 17-19 min., $0.32 + 0.08$ sec.; 26-28 min., $0.32 + 0.08$ sec.
 $- 0.07$
 $- 0.08$
 $- 0.10$

(b) *An aiming test*

Approximately constant levels of performance were obtained in other mechanically recorded experiments. The results in another type of experiment—the pricking with free arm movement of a hole 2.74 mm.

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in diameter with a metal point 1.06 mm. in diameter cleanly every second—under different concentrations of nitrous oxide are given in Table III. The failures are given in percentages; the 'normal' percentage of failures before the experiments began was 18.

These experiments further illustrate the slight improvement from the worst performance obtained, which is an almost constant result of different classes of work occurring with degrees of intoxication up to a moderately severe type. Tapping and typewriting experiments showed fundamentally the same results. Similar records were obtained with all the substances employed. Thus mechanically recorded experiments seem to show that,

Table III. *Pricking experiments under nitrous oxide intoxication*

Time after com- mencing nitrous oxide inhalation min.	Percentage failures			
	10 % N ₂ O	20 % N ₂ O	30 % N ₂ O	40 % N ₂ O
1-2	23	38	56	55
6-7	38	53	62	87
11-12	32	55	68	92
18-19	—	50	60	88

provided interest and awareness could be maintained, an approximately uniform performance could be obtained for the same degree of intoxication induced by an indifferent gas. The main object of the present paper is to consider the effect of these varying levels of intoxication on the act of remembering; and as a guide to the degrees of intoxication employed the effect on Simple Reaction Times may be taken. Two main groups may for convenience be distinguished: (i) Moderate intoxication, showing an increase of S.R.T.'s of 15-25 per cent, produced by concentrations up to 30 per cent nitrous oxide, 20 per cent ethylene, 15 per cent acetylene, 5 per cent methyl ether, or 1.9 per cent ethyl chloride. The higher concentrations in the group caused slight muscular incoordination. (ii) Severe intoxication in which the S.R.T.'s were increased 25-40 per cent, produced by concentrations up to 45 per cent nitrous oxide, 35 per cent ethylene, 25 per cent acetylene, 10 per cent methyl ether, or 2.5 per cent ethyl chloride.

III. EXPERIMENTS ON REMEMBERING: THE TASKS ASSIGNED

Owing to the severe degrees of intoxication investigated the tasks assigned had to be of a simple nature. They were recall of:

- (i) The Lord's Prayer.
- (ii) Ten simple questions set by my assistant Dr B. M. Davidson (the agreed answers are given where necessary in brackets): (1) The time of

day. (2) The day of the month. (3) Who is the Prime Minister? (4) Where is St Andrews? (5) Where does King George live? (Buckingham Palace.) (6) What is Christmas fare? (7) Name three rivers in Scotland. (8) Give the capital of England. (9) Where have you been in Wales? (Barmouth; Llandudno.) (10) How would you get to Queen's Cross (near my home).

(iii) Word series of varying difficulty; and of proverbs and simple phrases. Occasionally other more difficult tasks were employed.

The use of a mouthpiece made it desirable for answers to be written, a method which not only afforded the advantage of a permanent record but one which also gave additional information as to mistakes, in-coordination, etc. Controls were not taken in this series of experiments, but some had been taken for previous experiments with pure nitrous oxide. Of eighteen words in pairs read over once, fourteen were written in the next 3 min., and after nine professional words had been read out they were written correctly in 75 sec. This result probably represents my capabilities. I have no special facility in memorizing; not one of the many speeches I have prepared has approached in delivery reasonably closely to the written word.

IV. RESULTS

(a) *With moderate intoxication*

Recall of the Lord's Prayer, or of the questions and their order, was not affected by intoxication less than that produced by 20 per cent nitrous oxide or 10 per cent acetylene. With increasing concentrations a longer time than normally was employed in performing the tasks and indications of troublesome points began to occur. Thus 'Lead us not into temptation' was momentarily left out of the Prayer in two experiments, the first 's' in 'trespass' in two other experiments, and the answer to the fifth question was relegated to the end in another experiment. Even moderate intoxication (30 per cent nitrous oxide, etc.) had surprisingly little effect on the writing of the Lord's Prayer or the series of questions. The Prayer was written correctly and without hesitation under 30 per cent nitrous oxide although previously 70 per cent had been inhaled to unconsciousness, followed by 50 per cent in an unbroken experiment. The writing of it, however, took 5 min. as compared with $2\frac{1}{2}$ min. normally. The commonest, often the sole mistake, in the Prayer was the omission or duplication of an 's' in 'trespass' or 'trespasses'; on one occasion an additional 'l' was added to 'will'.

The series of questions was generally answered correctly and in order, but again the answers took longer than normally to write. In one

experiment, when 30 per cent nitrous oxide was being inhaled, I continued writing the series of questions until stopped. They were written three times correctly. At the fourth attempt the sixth and eighth questions were omitted, and at the fifth attempt the seventh and eighth questions.

(b) *With severe intoxication*

With concentrations producing the less powerful effects of this group the Lord's Prayer was usually written correctly except for the duplication of 's' of 'trespass' or 'trespasses' and similar mistakes. Under 40 per cent nitrous oxide 'come' was written 'conne' and in one of the questions 'union' became 'uniion'. Barmouth received an additional 't' and 'Llandudno' an additional 'n'. During an inhalation of this strength a paragraph of German was offered for translation. It was translated more literally than it would have been ordinarily, the German order of words being more closely followed. 'Nuchternwert' was translated 'hungry condition'. The Lord's Prayer was written even during the inhalation of 50 per cent nitrous oxide, which followed 70 per cent to unconsciousness. There was some hesitation after 'as it is in Heaven', which was followed by 'Hallowed be', but this was crossed out and the Prayer was finished correctly. The same momentary mistake occurred during the inhalation of 2.5 per cent ethyl chloride. It may be mentioned that in the experiment with nitrous oxide just referred to the phrase 'Home, Sweet Home' was given before the commencement of the 70 per cent inhalation for recall as soon after the return from unconsciousness as possible. It was remembered 7 min. after the change to 50 per cent nitrous oxide.

With deeper intoxication the two chief stumbling blocks in the Prayer were at 'Lead us not into temptation' and 'Give us this day our daily bread', and in this order. Reduplication of letters was almost constant but was less marked in the Prayer and questions than in other tasks. In the written Prayer, commenced 3 min. after an unbroken change from 70 per cent nitrous oxide to one of 50 per cent, there were duplications in 'hallowed', 'trespass' and 'trespasses', 'all' and 'evil'; and 'forogive' was written for 'forgive'. Above this 50 per cent nitrous oxide intoxication the remembrance of the Prayer began to break down. The earliest part of the Prayer was the most fixed in memory. The end portion, even the 'Amen', was never remembered if memory for the middle part failed. When remembrance of all beyond 'Thy will be done' had gone, the earlier part of the Prayer could still be repeated. Memory of the first line began to fail at an intoxication produced by 60 per cent nitrous oxide.

Most of the answers to the questions were remembered to a degree of intoxication when the writing became almost indecipherable from incoordination. They were remembered under 50 per cent nitrous oxide but the order of recall was erratic. The answer to the fifth question (Buckingham Palace) appears to have offered the greatest difficulty. Soon after a change from 70 per cent to one of 50 per cent nitrous oxide the questions could not be remembered, but when dictated the answers were given correctly. Reduplication of letters was about equal to that of the Lord's Prayer. It was more frequent in replies to incidental questions. As examples the following replies to unexpected questions given during the inhalation of 25 per cent acetylene will suffice. The first question was asked 14 min. after the inhalation commenced. (1) Q. "What are your views on the Scout movement?" A. "I believe the Boy's Sscouts movements is a most excellent idea. It stimulates ideas into coloniiaal assootiation." (The reply took 5-40 sec. to write.) (2) Q. "Give your views on the action of anaesthetics." A. "Who ask something about anaeesthetics. I tthink tthat anaesthetics are duu to the differenece exhibited in the Overton-Meyer Hypotthesis." (The answer took 5 min.) (3) Q. "What are your views on Colour Photography?" A. "You are interested in coolour FFo Photograraghy. I tthink it is posissible the procore procoph." At this point I appear to have wandered. Later, I must have recovered somewhat for I wrote, "My views on colour photograaphy are...that the usually accepted veiew of the Young Hering theory is essestialllly right." It is interesting to note that in typewriting experiments the mistakes made were not those of reduplication.

A considerable number of experiments were made on the recall of word series and phrases. Only a few examples need be given. During an inhalation of 25 per cent acetylene, when the Lord's Prayer could only be remembered to 'Thy will be done', a sentence 'the students will soon be back' was read out, followed by the associated words, at 15 sec. intervals—spring, country, green, trees, nests, birds, grass, lambs, flowers. After an interval of $1\frac{1}{2}$ min. I remarked that I could not remember anything, but on being pressed wrote—flowers, grass, green, nests, birds. The inhalation was stopped a minute later. Four minutes afterwards the word series written was—country, green, trees, nests, birds. At the same time the sentence given was remembered. In another experiment, in which 25 per cent acetylene was being inhaled, the phrase 'if wishes were horses beggars would ride' was given out before the experiment and the following words, at 15 sec. intervals, were read out

during the induction of the intoxication: rain, town, horse, grass, seat, clock, pill, fish, life, dream, blue, egg, word, city, war, lamp, cloud, coat, bag, back. The inhalation was continued for 8 min. The protocol of recall, the times given being those from the cessation of the inhalation, was as follows: 14 min.: remembered that a sentence proverbial in nature had been given; remembered cloud, frog (incorrect), back, time (incorrect), clock; also that all the words given were three to five letter words. 25 min.: remembered 'something about beggar's drive'. 31 min.: remembered 'clouds, woods (given on previous day), clock or time'. 33 min.: 'fish'? 35 min.: 'seat'? 38 min.: 'beggars must drive'. 66 min.: remembered the proverb. Continued thought had brought 'horses' into consciousness and this was quickly followed by 'wishes' and the complete sentence. 100 min.: remembered 'clouds, frog, trees, clock, table, chair'. Only two words are correct, but all the others had been given in previous experiments.

In a less degree of severe intoxication—the inhalation of 20 per cent acetylene—eleven of the twenty words given were remembered. The dictated words were: frog, star, wood, sea, table, spider, time-marker, bottle, metronome, girl, telephone, night, drum, church, bed, tree, book, chair, man, train. At the eleventh word I indicated that I could not remember anything that had gone before. The inhalation continued for $7\frac{1}{2}$ min., and $2\frac{1}{2}$ min. after its cessation I was unable to stand without support. Six minutes later, frog, star, sea, time-marker, metronome, girl, bed, telephone, tree, were remembered, and an hour later ink (incorrect), table and wood were added. Generally speaking, familiar professional words were most easily remembered; and when the word series was commenced with the inhalation, the earlier words, heard under slighter degrees of intoxication, were remembered better than those at the end of the series. Words not remembered may sometimes be picked out from a list containing them. In an experiment, in which 40 per cent nitrous oxide was being inhaled, the following words—pain, wise, pear, white, pink—were given at 15 sec. intervals during the last $1\frac{1}{2}$ min. of the inhalation. Two words, pain and wise, were remembered 25 min. after its cessation; the rest were picked out from a list of twenty words offered next day.

During an inhalation of 50 per cent nitrous oxide I had stated $11\frac{1}{2}$ min. after its commencement that I was not thinking of anything. On being asked to write down any names that came into my head I wrote, without hesitation, cat, crocodile, horse, ass, bayonet, knife, handkerchief, property, head, wire, electric light, heat, Joule's equivalent. A knife,

handkerchief, wire and electric light were within my purview so that most of the series of words seems to have been inspired by visual stimuli.

A few additional items bearing on remembering may be mentioned. After an hour's inhalation of 15 per cent acetylene the sums— $7\frac{4}{7} \div 8\frac{5}{8}$ and $14\frac{5}{8} \times 12\frac{7}{8}$ —were done correctly, and during the inhalation of 34 per cent ethylene $0.687 \div 948$ was accurately worked, but an additional 0 crept into the answer. With somewhat larger concentrations of ethylene, namely 40 per cent, multiplication could be done correctly but not addition. The concentration of ethyl chloride that showed this distinction was 1.9 per cent. The calculations were made after the manner of a child in an elementary school. During the inhalation of 50 per cent ethylene no calculations whatever could be done.

The estimation of time periods of more than a second was lengthened during intoxication. The estimation of 10 sec. periods, for example, was increased under 20 per cent nitrous oxide by 65 per cent, under 30 per cent nitrous oxide by 106 per cent, and under 40 per cent nitrous oxide by 121 per cent. Similar prolongation was found in the anoxaemia produced by nitrogen inhalations.

No comment is offered on the experiments which have been described. The object of the paper is to draw attention to a method of experimentation which, since it seems to produce fairly uniform mental levels, may offer help in the investigation of some psychological problems. A nitrous oxide-oxygen mixture is the most agreeable. With the smaller concentrations no unpleasant effects beyond a slight feeling of suffocation and occasionally slight nausea were experienced. In severe intoxication excitement may occur, but it is less after repeated experience of the inhalations and when attention is maintained.

(Manuscript received 12 April 1936)

SOME RECENT EXPERIMENTS BEARING ON 'INTERNAL INHIBITION'¹

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I. INTRODUCTION

THE notion of *Inhibition*, as opposed to that of *Fatigue*, has for some time been part of the conceptual stock-in-trade of psychology and physiology, and has been used in the description and interpretation of a wide range of phenomena. To name only a few, we may mention bodily fatigue, mental fatigue, memory, perception, psycho-pathological states, spinal reflexes and conditioned reflexes. And it would be possible to name as many other topics in which the concept might find application with as great a degree of justification as it possesses in these.

It happens that lately there have been carried out in the Cambridge Psychological Laboratory a number of experiments which, although directed at their outset towards a variety of ends, and employing a variety of means, have all afforded in their outcome suggestions of a similar nature regarding processes that underlie the phenomena observed. In this experimental work I can claim only a small part, but in describing, and in attempting to interpret, the experiments done by my colleagues, I alone must bear responsibility.

Briefly, each of the experiments to be described—experiments upon topics as various as experimental deafness in human beings, cessation of

¹ Read at a joint meeting of the British and Cambridge Psychological Societies, Cambridge, 28 November 1936. As regards both form and matter, no attempt has been made to eradicate more than the most glaring evidence that the paper was intended to be read aloud rather than printed.

eating in rats, habituation of the eye-blink response in man, and the behaviour of the last abdominal ganglion in the cockroach—each of these experiments seems to provide evidence of an underlying process or state essentially similar to that called by Pavlov⁽⁵⁾ ‘Internal Inhibition’. One important characteristic of Pavlov’s internal inhibition is its tendency to be dispersed by stimuli which, although insignificant in themselves, attain in virtue of the uniform constant sensory background imposed by experimental conditions a prominence sufficient to cause a mild degree of surprise or even of excitement in the organism. The fact that a state of internal inhibition has been dispersed is to be inferred from the renewed occurrence of the response previously inhibited. In our own experiments such *disinhibition*, as Pavlov calls it, has been found associated with the various types of cessation or diminution of activity we have studied. And it is largely for this reason that we have come imperceptibly, and perhaps uncritically, in thought and conversation, to interpret our results in these terms. The notion of disinhibition has been bandied about with a freedom which the facts, upon closer examination, might disapprove; and has presented itself to the eye of optimism as a panacea for all ills, from Freudian repressions to bodily fatigue! I should like, then, very tentatively to try and analyse these findings and determine the measure in which they demand, or are susceptible of, explanation in similar or identical terms. I shall first recall the circumstances in which Pavlov’s internal inhibition is produced. I shall then describe the experiments in question, and finally I shall try to bring them into line with one another, and with Pavlov.

II. PAVLOV’S ‘INTERNAL INHIBITION’

Everybody is familiar with Pavlov’s dog. This animal, when the arrival of its meal was heralded by the sound of a bell, would eventually take to dribbling at the mouth so soon as the bell was rung, before the arrival of the meal. This is the establishment of a conditioned reflex. But the continuance of this behaviour was dependent upon the subsequent arrival of the meal on each occasion; and if on a number of occasions the dog was disappointed of his dinner, and the bell became as sounding brass or a tinkling cymbal, signifying nothing, he would cease to salivate at the sound of it. This dying away of the response with lack of reinforcement of the conditioned reflex was termed by Pavlov *Experimental Extinction*. It could not be conceived as the mere disappearance of a habit, in the sense that the structure or function built up with the formation of the habit to serve as its organic basis is simply broken down

again with its dying away. So much is evident from the fact that in certain circumstances the response may reappear without any repetition of the training by which the habit was first established. The response will, for instance, reappear spontaneously after a period of rest varying in different reflexes from a few minutes to some hours. Again, an extraneous irrelevant stimulus such as the movement of a shadow across the visual field will produce a temporary recovery of the response. These facts demand that we view internal inhibition as an active process or state imposing inactivity upon a mechanism which remains intact throughout.

With *Experimental Extinction* are grouped by Pavlov three further types of internal inhibition—*Conditioned Inhibition*, *Differential Inhibition* and *Inhibition of Delay*. In each of these we find the possibility of disinhibition, together with some of a number of other characteristics which we shall enumerate. Before doing this we must call attention to two features of the experimental method used in work upon internal inhibition. These will be relevant to our later discussion. We must also mention the reasons for supposing that conditioned reflexes and their inhibition and disinhibition are, in general, especially to be associated with the functioning of the cortex.

The relevant features of the experimental method are the following:

(1) The dog is placed in a room by itself. It is held in a sling in order to maintain a constant position and posture. By preliminary training it is habituated to the experimental situation. In Pavlov's experiments, the experimental rooms were sound-proofed, and precautions were taken to ensure that extraneous physical influences, such as vibrations and draughts, did not penetrate into the dog's environment. By these means it is sought to confine the total sensory background for the dog to one as uniform and simple as possible.

(2) The stimuli used in the establishment and the inhibition of conditioned reflexes are spaced at fairly regular intervals. In general these were of the order of a few minutes.

As to the reasons leading us to suppose that conditioned reflexes are cortical phenomena, their very nature is, in the first place, suggestive. And complete extirpation of the cortex, according to the experiments of Pavlov⁽⁵⁾ and more recently of Culler & Mettler⁽¹⁾, results in the loss both of previously acquired conditioned reflexes, and of the power to acquire new ones.¹

We turn now to the characteristics of the state of internal inhibition.

¹ The latest results in this field, however, indicate that conditioned responses can be set up in a decorticated animal. See Girden *et al.* (10).

Pavlov's own definition is given as follows (5, p. 48): "In the second type of inhibition, which may be termed internal inhibition, the positive conditioned stimulus itself becomes, under definite conditions, negative or inhibitory; it now evokes in the cells of the cortex a process of inhibition instead of the usual excitation." The characteristics of the state are the following. In the first place it is gradually aroused when the animal is subjected to a fairly regularly spaced series of stimuli which previously evoked a response but which are not reinforced. Secondly, if the circumstances in which the state is aroused are maintained after the response has disappeared completely—if the series of stimuli without reinforcement is continued—the inhibitory state grows more profound in that the response can be restored, whether by spontaneous recovery, disinhibition, or retraining, only with increasing difficulty. This effect is called by Pavlov *Extinction beyond Zero*. Thirdly, so far as inhibition by experimental extinction is concerned, it would seem that the state, at first confined to the specific response in question, spreads or irradiates to others involving first those conditioned to stimuli in the same sensory mode, and later those belonging to other modes. Eventually inhibition spreads over the whole cortex and the dog is liable to go to sleep. Fourthly, the state is one capable of temporary dispersion, by spontaneous recovery, or by disinhibition. Lastly, after its disappearance, it leaves behind it an increased tendency towards its establishment upon future occasions.

III. EXPERIMENTAL DEAFNESS

We come now to the experiments carried out at Cambridge. First there is an experiment, by Rawdon-Smith (6, 7, 8), whose object was, in the first instance, to determine whether or not the auditory apparatus in man is subject to fatigue. It was evident from inspection of previous work on the subject that none but the most careful and precise control of all the relevant circumstances would serve if the existence of the phenomenon were to be definitely proved or disproved. Accordingly, among other precautions, the subject was placed in a sound-proof room. He was seated comfortably in a chair, and the room was lighted, though not brilliantly. For the method of controlling the fatiguing tone, and of determining the auditory threshold, reference must be made to the original papers (6, 8). The experimental procedure was briefly as follows: The normal lower auditory threshold for a few chosen frequencies was determined for both ears, the intensity of the stimulus being alterable in steps of 2 db. The scatter of these readings was small, usually about

± 2 db. The subject was then *mono-aurally* subjected to a pure tone of high intensity for a period which, in the majority of cases, was 2 min. The intensity of this tone was generally 100 db. above the normal threshold. That is, in so far as it is possible to compare pure tones with cacophonous noises, the fatiguing tone would sound roughly as loud as the racket created by four men beating a steel plate 5 ft. away from one's ear. The frequency of the tone was in some cases low, 400 C.P.S., and in others high, 1000 or 2000 C.P.S. What remained of the subject's hearing was then re-tested by determining the threshold for both ears.

The results of this experiment were as follows: there was a hearing loss, in general, in *both* ears, with a fatiguing note of high frequency. This loss in some cases amounted to 20 db.; in a few to 50 db. With a fatiguing note of low frequency, the losses were much smaller for the same intensity, and in some cases there was no loss at all. The stimulated ear generally suffered a greater loss than did the unstimulated. In no case was there a significant loss in either ear unless the intensity of the fatiguing tone was greater than 80 db. above the threshold. The hearing loss, though indisputable in both ears, was extremely variable in amount, both as between different subjects and as between different experiments on the same subject. The effect was in general transient, passing off in a few minutes, but sometimes persisting for a matter of hours.

Now what conclusions can be drawn regarding the locus of this phenomenon? It can be shown that under the conditions of the experiment the stimulation of the nominally unstimulated ear, due to the escape of sound round the edges of the telephone, bone conduction, etc., is very small. Certainly in no case is it conceivable that the nominally unstimulated ear was stimulated at any intensity even remotely approaching the 90 db. above threshold required to produce fatigue by direct stimulation. The hearing loss in the unstimulated ear must, then, be due to the intervention of some central factor. This being granted, the problem remains of whether the locus of the effect is the cortex, or some lower centre. That it is in fact cortical is suggested by the variability and by the individual differences displayed. At this point it was suggested that if the effect *were* cortical, it might be in the nature rather of inhibition than fatigue. The obvious test for this was to try to disinhibit it, and in this Rawdon-Smith^(7, 8) was successful. The fatigued threshold having been determined, the light in the sound-proof room was momentarily extinguished, leaving the subject for 2 sec. in complete darkness. The threshold was then re-determined, and was found to have fallen again to normal, or near to normal. The disinhibitory effect of this unexpected

stimulus was transient, and the threshold of the undisturbed subject would again rise to the fatigued level. In favourable circumstances, the experiment could be repeated several times before the threshold spontaneously returned to normal. It was established that such a stimulus as this was without effect upon the normal threshold.

There would seem no doubt that part of the hearing loss in this *Experimental Deafness* is occasioned by the arousal of some form of inhibitory state of the cortex. This inhibitory state is produced by the continued action of a stimulus of considerable intensity, upon a subject whose sensory background was of a relatively simple and uniform nature. It is transient, passing off spontaneously, and is subject to disinhibition by a stimulus which, although itself of a mild nature, is calculated in the circumstances to occasion a greater or less degree of surprise.

IV. SATIATION OF DRIVES

The second experiment is of a totally different character. Engaged in watching the copulation behaviour of rats, Drew noticed that when satiation was apparently attained and copulation ceased, a mild stimulus such as that produced by scratching the bars of the cage would bring about renewed activity. (In rats copulation behaviour characteristically includes a succession of separate sexual acts.) Drew⁽³⁾ continued the study of this phenomenon in the food drive. He found that rats which have ceased to eat, but have further food at their disposal, will again begin under a variety of conditions. In the first place, stimuli such as the sound of a water tap running, or the simultaneous flashing of a flash-lamp bulb and the buzzing of a buzzer, will bring about renewal of eating. Stimuli such as these, if *too* intense, fail to have this effect. Instead, the rat will manifest signs of fear. The correct intensity was that which would cause signs of surprise or expectation, but not alarm. Secondly, the introduction of an animal of the opposite sex into the neighbourhood has the same effect. Thirdly, conditions named by Drew *Situational Stimuli* will also induce renewed eating. Typical of these is the removal of the food to some place that demands of the rat the expenditure of a certain amount of effort in its recovery.

The implications of experiments such as these are, obviously, highly important, but at present necessarily obscure. The light they may be expected to throw upon the nature of drives and incentives does not concern us here. But in general it would seem that we must regard the satiation of a drive, not as the gradual disappearance of an urge, or the

slackening of some internal tension, but rather in terms of a positive inhibitory process preventing the operation of a mechanism which otherwise would remain active—to the manifest detriment of the organism. I will not insist on such an interpretation. There are evidently a number of complexities to the whole question which as yet are unexamined.

V. HABITUATION OF THE BLINK RESPONSE

The third experiment we have to consider is one performed, using a very inadequate technique, I fear,¹ by myself. Here we are concerned with the habituation or negative adaptation of the eye-blink response to auditory stimuli. It has, for some time, been known that certain simple reflex-like reactions tend progressively to diminish in amplitude of response upon repeated regular presentation of the appropriate stimuli. A number of findings of this nature have been obtained with lower animals. Humphrey⁽⁴⁾ obtained a similar result for the leg-retraction response of the tortoise to the stimulus of tapping its shell. The evidence obtained by Dodge⁽²⁾ upon variability of various human reactions such as the knee-jerk and eye-blink shows incidentally that the response to a second stimulus is noticeably smaller than that to the first. The latent period of the reaction likewise increases.

By attaching one end of a thread to the eyelid of a human subject and the other to a light lever marking on a smoked drum, it was possible to obtain some evidence regarding the nature of the response to a loud click produced in telephones worn by the subject. The interval between clicks was 2 sec. Using what was really a very loud click indeed (so much so that it was almost painful), the characteristic record obtained was one of a somewhat irregular oscillation of the amplitude of lid closure, never reaching complete cessation of lid movement. With a somewhat less loud click, the response would die away more or less quickly, that is within seven or eight stimuli. From this intensity of stimulus, down to that which would evoke no recordable lid closure at all on the first stimulus, fewer and fewer stimuli were required to produce extinction of the response.

Like the experimental extinction of a conditioned reflex, this inhibition of the blink response may be disinhibited temporarily by an unexpected stimulus. In my experiment the stimuli used for this purpose were the flashing of a light, a sound stimulus consisting of an impure tone of about 500 C.P.S. fed for about a quarter of a second into the telephone between

¹ Apparatus for a very much more exact investigation has now been built.

two clicks, and the introduction between two ordinary clicks of a third similar one. Neither of the first two stimuli were sufficiently intense to evoke the blink themselves. In the last case, it generally happened that no response was obtained to the extra stimulus, the return occurring first at the next click afterwards. Disinhibition was transient. It was noticeably easier to disinhibit a state of inhibition that had been produced by strong stimulus than one produced by a weak. This suggests that the stronger stimulus not only requires a larger number of repetitions to induce inhibition, but also induces a state less stable than that induced by a weaker. This may explain the irregular, oscillatory character of the response to very strong stimuli. It might be suggested that a state of inhibition starts to set in, resulting in a partial diminution of response, but that this state is so unstable that it is at the mercy of the least disturbance, and thus becomes disinhibited by extraneous sounds, or even, one may suspect, by involuntary eye movements of the subject himself. The last point worth mentioning in connection with this experiment is that the production of states of inhibition at frequent intervals within half an hour in one subject seemed to increase the ease with which the state might be aroused. This may be compared with the finding of Dodge⁽²⁾, whose observations extended over 9 months or so. The response of subjects examined daily during this period showed a marked decrease to the same stimulus at the end of the period.

VI. BEHAVIOUR OF THE LAST ABDOMINAL GANGLION IN THE COCKROACH

Lastly we come to another experiment by Rawdon-Smith in collaboration with R. J. Pumphrey⁽⁹⁾. Using a technique we need not describe, they have been able to record simultaneously the impulses entering and those leaving the last abdominal ganglion of the American cockroach. The impulses entering the ganglion were produced artificially by direct electrical stimulation. When a regularly spaced series of impulses is sent into the ganglion, those leaving it are at first regularly spaced also, one impulse leaving for each coming in. But after a certain time has elapsed, the impulses leaving the ganglion become irregular. Finally the post-ganglionic nerve shows only occasional discharges. The response never, however, disappears completely. When the post-ganglionic discharge has died away as far as it will, it is possible to bring it back temporarily by either of two means, namely a single stimulus interposed between two of the regular series, or increasing the frequency of the regular series.

Further, the disappearance of the response will not take place unless the stimuli are regularly spaced.

With regard to the relation of this experiment to those previously described, I will not say more than that it *may* provide us with an extremely simple analogue of processes we have studied at higher levels. It may be, and it probably is, the case that states of inhibition at these higher levels are aroused by mechanisms very different from those at play in the cockroach's ganglion. But it is a source of encouragement, when we are attempting to disentangle the exasperatingly complicated processes in the cortex of man and other mammals, to reflect that processes at least superficially similar do occur at the lowest conceivable level. In considering internal inhibition as a whole we shall do well to keep in mind the phenomena discovered by Pumphrey & Rawdon-Smith, together with the very ingenious explanation of the observed facts developed by them upon quite simple and acceptable assumptions.

VII. DISCUSSION

That completes the experimental evidence to be considered. We now turn to the dangerous, but delightful task of attempting to put some of the facts in their proper places. The range of possible considerations is very wide, and it is necessary to limit the scope of ours somewhat. In the first place, even were I able to do so, I would not indulge in any elaborate Sherringtonian analysis of the neural aspect of the processes with which we have to deal. The few facts at present at our disposal would render any such undertaking entirely profitless. Secondly, except in so far as I shall briefly consider the nature of reinforcement, I shall not attempt to deal with the various kinds of internal inhibition in their aspect of being biologically useful to the organism. Thirdly, it is my intention to deal only with the *conditions that arouse* states of inhibition, and it will be possible to consider these only very superficially. I shall make no attempt either to analyse the character of the state once it is aroused, or to relate the instances of it found in our experiments to those which certainly seem to play an important part in our mental life as a whole.

In the first place there is a distinction I should like to draw, and to underline. It is this. Ever since the discovery of conditioned reflexes, and their inhibition, there have been schools of thought the tendency, if not endeavour, of which has been to see in these effects the means of explaining the intimate mechanics of cortical function. Thus processes of

learning are to be viewed as the establishment of unbelievably complicated systems of conditioned reflexes. Differential inhibition is the archetype of all discrimination. And, in fact, all the complexity of behaviour is to be mirrored in a cortical pattern of excitations and inhibitions. It would be unwise to dispute the correctness which some such view as this may possess in the remote future. But, for the present, it is possible to indicate another, rather more humble, line of thought about processes of internal inhibition. Let us consider internal inhibition not as one of the *atomic* processes of cortical activity, but simply as a process or state which may, as a mere matter of fact, in a variety of different circumstances, descend upon any one of a number of different cortical activities. And let us, by studying it in as simple conditions as possible, try to determine the essential conditions of its arousal and dispersion. By doing so we may make no immediate contribution to the understanding of intimate cortical mechanisms. On the other hand it would seem plain that at every level of our mental life, states suspiciously reminiscent of internal inhibition constantly play a part. And it would be interesting and valuable if we could trace the conditions producing these states, from those which stop us blinking unnecessarily, to those which unnecessarily stop us thinking. If we cannot hope to find out *how* the brain *does* work, we may at least discover something about *why* it sometimes *doesn't*.

We may carry this argument a stage further, and suggest that the true interest of conditioned reflexes lies not so much in the fact and manner of their establishment, as in the opportunity they afford for the study of inhibition in exceptionally simple and favourable circumstances. On such a view one might hold that there is no intrinsic, but only a circumstantial, connection between the establishment and the extinction of a conditioned reflex. Pavlov, in fact, simply happened to study inhibition in a response which, as a matter of fact, had been built up by a process of conditioning.

In so far as the conditions arousing inhibition in general are concerned, our experiments fall evidently into two classes, namely those in which there was repetition of discrete stimuli at regular intervals, and those in which there was a single stimulus, or a single situation, operating. And we have first to consider how far *regularity* of stimulus is an important feature of the experimental conditions in the first class, and how far the discrepancy between the two classes in this respect must force us to take different views regarding the nature of the states produced. We have seen that disturbance either of the regular series of stimuli by the introduction of an extra member, or of the regular background upon which

this regular series is superimposed (by the occurrence of an unexpected stimulus), will upset the state of adaptation of the blink response. Whether or not the first of these two effects is found in the process of extinguishing a conditioned reflex, I do not know.

Now we may take one of two views about the part played by regularity. It might be either a *sufficient*, or only a *favourable*, condition of the production of an inhibitory state. In the first case, we might suppose that a regular series of stimuli exercises, *per se*, a kind of soporific effect upon the cortex. If this were the case, it would be the regularity itself of the series, rather than the nature of its members, that is effective. And it would seem to follow that *all* regular series of stimuli, regardless of their intensity, would exercise an inhibitory effect upon *all* responses. This is plainly not the case. There are series to which no response is elicited at all, and there are other series to which continued response is obtained. As to those stimuli which do not bring about any response, it is, of course, possible that they do in fact produce a state of inhibition, the existence of which we might demonstrate if we could give the organism a test-stimulus which in normal circumstances *would* bring about a response. But almost certainly such a stimulus would itself disinhibit the inhibition. In connexion with the eye-blink, I have already suggested a view about those series of stimuli which evoke continued response.

It would seem then that the other view, that regularity of stimuli is only a favourable, and not in itself a sufficient condition for inhibition, is more likely to be true. And the question arises—why is regularity favourable to the arousal of the state? I suggest the following view about this: *Fundamentally, one important condition is uniformity of the total sensory field. Simple regular discrete changes in that field become accepted as uniformity after a certain lapse of time, but only on condition of their regularity.*

Now it might be argued that a sensory field without a series of clicks would, in spite of the clicks becoming a uniformity, nevertheless be at least as uniform as, and certainly simpler as far as the total number of elements it comprises is concerned, than one without such a series of clicks. But this does not follow. There is certainly competition between stimuli, and some may well mask others, *not necessarily because of their greater intensity, but because of their stronger coherence*. So it may happen that the uniformity of total environment, somatic and external, necessary to establish and maintain a state of inhibition is actually dependent upon the inclusion in that environment of a regular series of stimuli. If this is the case, we can see how an extra stimulus between two in the series,

or an irrelevant stimulus not belonging to the series at all, might act as a disinhibiting agent. Either simply raises the differentiation of the total environment to a point where the regular series of stimuli is unable to impose a sufficient uniformity.

In putting forward such a view as this we have, it is evident, separated the stimulus in its aspect of producing inhibition from the stimulus in its aspect of evoking response. That is, we have suggested that the power of the stimulus to produce inhibition is, in a certain measure at least, independent of its power to produce response. To a certain extent this duality of aspect is probably real. It simply happens that stimuli which are strong enough to impose sufficient uniformity on the sensory field to *produce* inhibition are also strong enough at first to evoke some response, and are therefore ones capable of *manifesting* inhibition.

This leads on to the question of the relative parts played by the stimulus itself, its lack of reinforcement (or whatever in effects other than conditioned reflexes is the analogue of reinforcement), and the response itself, in the production of the inhibitory state. Before dealing with this question, however, we have to consider those of the experiments described which fell into the second class. Here the conditions producing inhibition did not include a regularly spaced series of stimuli, but a continuous stimulus, or a more or less uniform situation. The two cases, that of experimental deafness, and that of drive satiation, must, I think, be treated separately.

First, then, as regards experimental deafness, we may suggest a possibility of bringing this into line with our fundamental condition of uniformity of sensory field. No one will deny that a very loud noise of constant character *does* impose a considerable measure of uniformity upon one's sensory field. But, it may be asked, why is such a *very* loud noise necessary, in view of the fact that a continuous noise should presumably impose a greater uniformity than an intermittent one, and reactions such as the eye-blink are so easily inhibited by intermittent noises? A possible answer to this is that the noise used by Rawdon-Smith may, in fact, have induced a proportionately profound degree of inhibition, but that the auditory threshold is, as indeed one might expect it to be, singularly impervious to the action of that inhibition. There may, then, be a more or less close relationship between the type of inhibition found in experimental deafness, and that found in habituation and in the extinction of a conditioned reflex.

Any optimism we may feel in regard to such a connection as this, however, must stand in contrast with the extreme difficulty of accounting

for drive satiation. In any case, it would seem extremely unlikely that the whole problem of the activation and satiation of drives can really be compassed in terms of inhibition and disinhibition alone. And I do not feel that in the present state of our ignorance regarding these and other factors, the subject should really be discussed in this connexion. We may, however, perhaps suggest, very tentatively, that any activity such as is involved in the unimpeded satisfaction of a drive may impose a uniformity upon the sensory environment of the animal. In ordinary terms we should certainly say that the animal's attention is fully *occupied* in eating. If, then, a uniformity is imposed upon the sensory field of the animal by this means, we might expect that continuance of the situation might lead to the establishment of a state of internal inhibition.¹

But there is a difficulty about such a view as this, in that the state of inhibition is not by any means so transient as are the other instances of it we have studied.

Lastly we have to consider the question of the relative parts played by the stimulus itself, by its lack of reinforcement and by the response itself, in the production of a state of inhibition. We have suggested that in so far as the stimulus does play a part, it is by imposing a uniformity upon the sensory field in virtue either of its intensity, or of its coherence. But this leaves open the question of how far it is the stimulus, *rather than the response*, that produces the states.

This question may well be asked in relation to Thorndike's *Law of Use*, which may be stated by saying that performing an action increases the tendency for it to be performed upon a future occasion. As Mr G. C. Grindley has pointed out to me, in so far as habituation is a simple form of learning process, it would appear to follow a law which is the exact converse of the *Law of Use*. Blinking on one occasion seems to decrease one's tendency to blink on another. As to this matter, I think it correct to say that although the giving of a response, and the nature and extent of that response, *may* be one of the factors concerned in the determination of the neural state which in its turn will in part determine the response to the next stimulus, yet it is, in the cases we have considered, primarily the stimulus and its characteristics that determine that neural state. And I say this for two reasons. In the first place, we have seen that in the extinction of the conditioned reflex (and I am inclined to think we shall find the same to be true of habituation), extinction proceeds beyond zero.

¹ Dr R. H. Thouless has pointed out to me a further possible analogy. In the extinction of a conditioned reflex, the animal tends finally to go to sleep. The same is true in the satisfaction of a drive!

Here the stimulus continues to exercise its effect at a time when no response at all is being elicited. Secondly, if the identification of the state of inhibition in experimental deafness with that in habituation is justified, we have plain evidence that it is the stimulus rather than the response that is the cause of inhibition. For in the conditions in which experimental deafness is produced, no response to the stimulus occurs at all.

About reinforcement, I shall ask only two questions. In the first place, what is the counterpart in habituation of reinforcement? In the second, how far does lack of reinforcement contribute towards the establishment of inhibition?

It is a differentiating characteristic of habituation that it occurs only to stimuli which, although they *might* have possessed biological significance for the organism, in fact do not. Thus we cease to blink at a series of loud sounds because these sounds are harmless, although they might have betokened some such event as the explosion of a bomb in the neighbourhood. But we continue to blink to a series of puffs of air directed into the eye because these might dry up and damage the cornea. It may be said, then, that if the response given on the occasion of the first stimulus satisfies, or tends to satisfy, some organic drive, then it will persist. If it does not, it will not. And such a view would bring habituation once more into line with the extinction of a conditioned reflex. The dog ceases to salivate at the sound of the bell if no food follows. Its response no longer tends towards the satisfaction of the food drive. Into the mechanisms by which a drive-satisfying response is functionally distinguished by the central nervous system from a non-drive-satisfying one, we cannot enquire here. They certainly cannot be formulated in terms of cognition and voluntary action.

As to the part played in bringing about inhibition by the failure of the response to satisfy a drive, it is plain that in conditioned reflex extinction it is highly important, since a regular series of stimuli *with* reinforcement will continue to evoke responses, and will fail to bring about inhibition. This does not, of course, prove that regularity of the stimuli is not also a necessary or favourable condition of inhibition even in this case. It is also evident, in some cases of habituation, that inhibition can be produced without regularity. In particular, the second stimulus no less than the later ones of a series will produce a response smaller than the first. In such cases it would seem that lack of reinforcement is the more important factor.

I wish, then, to emphasize these two conditions favourable to the production of a state of internal inhibition—(1) failure of the response to

satisfy a drive, (2) uniformity of the sensory field, a circumstance that may be imposed by a single stimulus of great intensity, by a series of intermittent stimuli regularly spaced, or perhaps by the activity involved in the free unhampered satisfaction of a drive. If I have treated more fully the second of these, it is because it is a condition perhaps less obvious, but more easily investigated than the first. What really happens inside an animal when the event we call the satisfaction of a drive occurs is a very obscure question. But we do know roughly what we mean when we say that a series of stimuli is regularly spaced.

VIII. SUMMARY

Some experiments in which, it is thought, states resembling Pavlov's 'Internal Inhibition' are produced are briefly described. The connexion between these various phenomena is discussed, and two conditions favourable to the production of internal inhibition are formulated. These are (1) uniformity of sensory field, and (2) failure of the response to satisfy a drive.

In conclusion I must express thanks to my colleagues at Cambridge, especially to Drs A. F. Rawdon-Smith and R. J. Pumphrey, and Mr G. C. Drew, who allowed me to make use of experimental results then unpublished; and to Prof. F. C. Bartlett, Dr Rawdon-Smith and Messrs O. L. Zangwill and G. C. Grindley for the indispensable conversations I have had with them on this subject. But for these, this paper would never have been written.

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A PSYCHOLOGICAL THEORY OF AESTHETIC VALUE

By A. H. B. ALLEN

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I. INTRODUCTION. THE GENERAL NATURE OF THE PROBLEM

THE experience of aesthetic value falls into two parts. There is first the subjective experience which some would describe as pleasure in the contemplation of a given object, though no doubt others would prefer to speak of a sense of goodness, satisfaction or 'worthwhileness'. Secondly, there is the factor which may be called the demand for conformity with a standard, the consciousness, often no doubt only obscurely present, that our satisfaction in the contemplation of the object is 'right' and that others 'ought' to agree with it. This 'oughtness' feeling is less central in the aesthetic than in the moral experience. But undoubtedly it is normally there. In itself it is primarily a subjective experience, but it can hardly be adequately discussed without some reference to the question whether it implies other than subjective experiences, from which its justification is derived. To do this would involve the introduction of philosophical or metaphysical considerations. The object of the present paper is to treat the subject-matter entirely on the plane of psychology and to exclude metaphysics. All that will be attempted is the more limited task of analysing the element of pleasantness or goodness in the aesthetic experience, regarded as a purely subjective experience, and not necessarily as having any implications beyond itself.

II. THE THEORY OF EMPATHY

Many such psychological accounts of the nature of the sense of beauty or of aesthetic value in general are already in existence. They differ widely among themselves, and within the limits of this paper it would be impossible even to attempt to mention them. The particular theory which I propose to take as a starting-point is that based on the phenomenon known as *Einfühlung* or Empathy. It may be considered to have attained the status of a certain orthodoxy and to be well known to readers of psychology. It is therefore unnecessary to describe it. Those in search of further information can be referred to the appropriate chapter in Listowel's work, where quotations from and references to the accepted authorities will be found(1). All that I would desire to point out is that what is called the Empathy theory should properly be considered as only a part of a wider theory of aesthetics. This wider view is one which must hold aesthetic enjoyment in general to depend on the fact that, when any emotional situation is described or depicted for us, we as onlookers share sympathetically in the appropriate emotion, and that thus we obtain, in a vicarious form, valuable experiences which would otherwise be missing from our lives. It is obvious that this view is primarily applicable only to the directly representational arts, such as literature, and painting in its imitative aspect. It is the special function of the Empathy theory to show that it can be applied to the arts usually called non-representational, music, architecture, and painting in its 'decorative' aspect. It does this by pointing to the tendency, apparently natural and automatic in the human mind, to project life and feeling into inanimate forms, or into the sound patterns of music, and thus showing that these so-called non-representational arts do in fact appear as able to suggest human experiences and human values.

The advocates of the theory would probably admit that other factors, besides that of Empathy, may form part of the total experience of aesthetic value. There may be, in the first place, the factor of mere sensory pleasure in the colours or tones, whether singly or in combination, which are the material of a total aesthetic form. But this element of sensory pleasure is clearly only an inessential part of aesthetic value. It may be entirely absent from some products to which such value is attributed, e.g. from works of literary art. There may be also a certain element of satisfaction given in the apprehension of form as such, in facility as contrasted with difficulty of comprehension. Any form which shows a regular scheme, easily grasped by the mind, yields a certain satisfaction,

whereas the irregular and planless is naturally bewildering and disappointing to us. On the other hand, mere regularity and a plain repetition of the already presented may be unsatisfactory, as being dull and monotonous, and giving no opportunity for the exercise of mental activity. Those forms are, as such, most satisfying which show an apparent multiplicity and diversity, but in which it is nevertheless possible to discern a governing principle or plan. In the consciousness of bringing order into, and unifying, the multiplicity there is given a feeling of ease and power. But though this may exist as an element in aesthetic satisfaction, we can see that it is not essential to it. Everyone feels at once that beauty is something different from logical coherence and intelligibility. For these are ends which are sought more by science than by art. Moreover a purely intellectual comprehension, giving rise to a feeling of power, is something cold and alien to its object. For that school of which we are speaking absorption in the object and sympathetic participation in its life remain the essential facts in aesthetic experience.

III. CRITICISMS OF THE EMPATHY THEORY

The criticism which is often directed against the Empathy theory is that it fails to find any really distinctive character for aesthetic value. The value of art would consist in the fact that by its imaginative creations it is able to give us satisfactory experiences which would otherwise be denied us. This vicarious satisfaction by means of the imagination would be nothing but a *pis aller*, the best substitute available when we cannot have the real thing, but only a second-hand affair after all. Thus critics remark: "The experiences we get from successfully riding a bicycle, which presumably cause pleasure and facilitate our vitality, could clearly be recalled by our projecting similar movements into lines and rhythms, and the resultant state would be neither more nor less aesthetic than the original one, except in virtue of its new origin in recall through projection. And such an origin is hardly more relevant to aesthetics than if the same state were to be recalled by internal stimuli in a dream."⁽²⁾ But may we not pursue this point further and ask whether after all some sort of a distinctive value may not be derived from the mere fact that the experiences are given in an ideal form? It is obvious indeed that in imagination we can surpass the performances of which we individually happen to be capable, taking part in romance or adventure either denied us by our circumstances or beyond our actual powers. In imagination we can, moreover, raise all valuable qualities to a higher power than any which we have ever actually known, filling

up the deficiencies and imperfections which we always seem to find in real life. But even if there may be some products of art which seek to give this sort of satisfaction, we cannot agree that in it is to be found the true nature of aesthetic value. To hold such a view would be still to make this value the same in kind as that of real life, only with a 'more so', the addition of something. Surely we feel intuitively that this is crude and unsatisfying and that art must have some more distinctive character than that. Can we not derive from the fact of ideality some really distinctive value, which is not that of mere exaggeration or addition? This is the question which I will now try to answer.

IV. THE SENSE OF FREEDOM IN AESTHETIC EXPERIENCE

Now in the first place there is a certain value or 'goodness' given through the sense of freedom in any activity of the mind. This is a fact of a very general kind, showing itself in many contexts in the mental life. Thus when the mind has set any end before itself, it feels itself, while that end is still unattained, as in a sense limited and oppressed. When the end has been attained, it feels as though affirmed and liberated. This feeling always exists in the same general form. It is a pure value of attainment, independent of the value of the end in its particular character. Again the same general feeling occurs as an element in all the many forms which the sense of power takes in our lives. The full consciousness of power arises when there is awareness of some actually opposing force which is being overcome. Included in this sense of power there are an exaltation and self-enhancement due to the fact that there has been a limitation or threatened limitation of the activity of the self by the opposing force and then this opposition has been overcome. In positive self-feeling, as it is often called, the feelings of release and freedom are essential factors. We have already noticed a particular instance of the sense of power in the satisfaction accompanying the comprehension of logically coherent form. In such a case the mind seems to itself to be delivered from a state of confusion and obstruction into one of clarity, ease and power. But this element in the mental life also shows itself in the sphere of play. Play is in essence an exercise of bodily or mental activity, taking place as a rule in the pattern of some instinct, but carried out when no longer necessary for self or racial preservation, or, as we often say, for some 'practical' end. The nutritive or sexual needs, for instance, show normally a certain forceful urgency, so that they dictate our actions with compulsion. Play is an extra to these needs, resulting from the overflow of abundant energy, and is therefore felt to

be to some extent free from their compulsory character. In virtue of this a value of freedom pertains to it. It would hardly be possible to discuss the nature of this mental experience further without a complete treatment of the psychology of the affective life. But it can be maintained that such a mental experience, that of a freedom experienced as valuable, does exist, that it is of an affective nature, and that it can be distinguished from pleasure and particularly from all sensory pleasure.

The most important aspect of art, according to the theory provisionally accepted, is that in respect of which it is an imaginative reproduction or suggestion of other experiences which already have a value of their own. It is therefore akin to play, inasmuch as it is an activity not dictated by the ordinary practical needs of life. Now all bodily and mental activities, even when carried out as play, have a setting in the material world, carry with them various bodily strains and tensions, and are very likely, after the original excitement, to involve a corresponding reaction and depression. All this means that the activity always takes place with some amount of friction and impediment. When the activity, whatever it may be, is one represented in idea only, these causes of friction are almost entirely eliminated. There are bodily accompaniments to the activity of the imagination, but the grosser strains which would exist in actual practice are absent. It is, partly at least, for this reason that, after a vivid anticipation, the actuality of some experience often appears as a disappointment.

Heard melodies are sweet, but those unheard
Are sweeter.

To take another instance, we may well hold it to be true that a keenly sympathetic spectator, watching some feat of grace and skill, say figure-skating or ballet-dancing, gets a purer, if not perhaps an intenser, enjoyment than the performer, who remains aware of the efforts and inner strains involved. In art there may be a freedom, not only from the compulsive urgency of nutritive and sexual needs, but also from bodily and sensory impediments, and in this feeling the self can enjoy its activity. There may then be a recreation of the other values which occur or are capable of occurring in life, with the added value of self-enhancement through freedom. It is in this latter element, I suggest, that the essence of the aesthetic experience is to be found. It is the same in its generic character as those other feelings of freedom or power, which have already been mentioned. But it takes its specific character because it is attained in a world of ideas.

By what actual means does art convey the feeling of freedom from instinctive and bodily limitations? Does any reconstruction in an imaginative form give aesthetic value? We can hardly answer this latter question with a direct affirmative. There are certain other characters required before an ideal reconstruction becomes art. In the first place there must be some awareness that the product is in some way a creation of something new, that is to say, is not merely a literal reproduction of what has been given by external perception, but adds something which proceeds from the artist himself. Given this awareness, there is an enhancement of selfhood by the feeling of power, a feeling of the same kind as that given by the fulfilment of what is often called the constructive instinct. This so-called instinct is probably, so far as man is concerned, an impulse to the enhancement of selfhood through the sense of being able to impress some permanent or semi-permanent alteration on external reality in accordance with the will of the self. The sense of creation is primarily felt by the artist himself. But we, the onlookers, are able in some degree to rehearse his experiences when we behold his work and so share in the original feeling of freedom and power. There must then in art be not only the feeling of freedom from instinctive and sensory compulsions, but also a feeling of the same order given through the sense of origination by the self. The two feelings being of the same order, it is natural that they should fuse into one whole. In the second place there must exist some awareness, probably as a rule in the background of consciousness, that the experience is related to the imaginary as contrasted with the real world. In the dream of sleep this awareness does not exist, because in it ordinarily there is no contrast of the imaginary with a given and imposed reality, and so there can be no feeling of freedom. In the dream the self appears either passive or subject to compulsion. In the third place the experiences must be expressed in some permanent or semi-permanent form, so that they can be repeated or communicated to others. All the essentials of aesthetic value may indeed be present in the day-dream of some individual, but they must perish unknown, unless they are embodied in some permanent form.

Given the fulfilment of these conditions, there are various subtle ways of conveying and enhancing the feeling of the spiritual freedom which is the essence of the aesthetic experience. In what follows I shall try to describe some of these in the spheres of the different arts, though within the limits of this article elaboration of details is hardly possible.

V. APPLICATION TO THE VARIOUS ARTS

(a) *The indirectly representational arts*

The broad distinction between the indirectly representational and the directly representational arts is still maintained. The former class includes the construction of sound patterns, i.e. music, and the construction of non-imitative visual patterns, a form of art for which we seem to have no one name, but of which architecture is usually considered the chief representative. Arts of this class have in themselves a more ideal character than those of the other, in the first place because in them the work of the artist is known immediately as a creation and not as a copy, and in the second place because meanings suggested indirectly are in the nature of the case a further stage removed from first-hand sensory experience. In music there are various elements which contribute to the total effect; but the essential fact is the suggestion of movement through sound patterns. Both in rhythm and in melody there may be given an ideal suggestion of movement forms, in which by sympathy we seem to partake. The intimate connexion between music and bodily movement is obvious. It is seen especially in the dance. But only comparatively crude and limited patterns can be adequately expressed conjointly in dance and music, for example in the waltz. In such cases what takes place is a sort of fusion of sensory and ideal elements. This is what seems to explain the intoxicating effect of dancing to music, though, owing to the presence of the sensory element, we can hardly call the feeling an aesthetic one. Attempts have also been made to translate even some of the more elaborate creations of music into dance, e.g. in the ballet, but never with any real success. The true function of music is to suggest an ideal world of movement transcending our ordinary experience and so to lift us above the limitations of the ordinary world of sense. Though we may think of analogues of these movements, such as the soaring flight of birds, the downward leaps and pauses of a waterfall, or even the rise and fall and the interplay of mysterious and vaguely personified forces, yet in fact there are no adequate descriptions of them to be given. Vernon Lee has obtained a large number of descriptions of their experiences from musicians or music lovers, many of which bear this interpretation. Here is a typical description given by one of those whom she calls 'listeners', i.e. those who declare their love for 'pure' music, music apart from any extraneous associations. "Il s'agit moins dans mon cas en général d'une émotion de la vie, ou même d'une signification au delà, que d'un plaisir spécial, propre à la musique,

plaisir très élevé, d'ailleurs, sorte d'état de contemplation active, impossible à analyser, que je définirais en gros comme une sublimation de moi, un allègement de l'être, une montée vers des régions inconnues, calmes et sereines, où le tumulte des passions ne gronde que pour y trouver l'apaisement." (3) Another speaks of a feeling of 'lévitation intérieure' (4). Within this total ideal sphere it is possible for some of the creations of music, because of their specific character, to emphasize and bring out the element of freedom more strongly than others. There is in some music a surge or 'carry forward', which seems to be that which critics mean by the 'lyrical' quality. In so far as it is analysable, it is the result of a subtle alternation of checks and releases in the rhythm, combined perhaps with the suggestion of a 'lilt' or rocking movement given by the pitch. It is possible to think of an analogy given by the comparison of some bodily movements with others. Our ordinary movements are constantly limited by the pull of gravitation and the friction of the earth's surface. Anything which enables us to overcome these impediments and to carry out swift gliding or swinging movements with the minimum of bodily effort gives us an exhilarating sense of freedom and power. Hence the fascination of mere speed in motoring, and of such exercises as tobogganing, skating, ski-ing. Herein there is an analogy, though of course it is an analogy only, to the difference in the quality of freedom between some of the patterns of music and others. At the same time it must not be supposed that the most valuable quality of music consists in mere swing and lilt. Some music gives the impression of a self-abandonment to such forms of movement, and then there is actually a diminution of the true feeling of freedom, for the mind seems to let itself go and surrender itself to an alien domination. That music is usually judged the higher which in the midst of its freedom preserves a feeling of restraint and self-mastery. As examples I would give, say, the Beethoven minuet as compared with a Strauss waltz, or perhaps a typical melody of Mozart as compared with one of Tschaikowsky.

In turning to the class of the non-imitative visual arts, we may observe firstly that any visual shape or pattern, if we surrender ourselves to its contemplation in a disinterested spirit, may give rise to the phenomenon of Empathy and appear to us as the expression of human ideas and values. Provided the values are real and the expression adequate, the mere fact of being given an apparently permanent embodiment may yield a certain feeling of freedom. The object, whether a building or some other structure, appears as the embodiment of some idea coloured by feeling, it may be one of harmonious poise and balance

of movements, one of massive strength and power of resistance, or one of distance, mystery and religious awe. These sentiments appear to us to be given a permanent form and body, so that we, in sharing sympathetically in them, seem ourselves to be released from the changing accidents of ordinary mortality, and to find rest in an ideal world of permanent values. We may feel with Keats that they

tease us out of thought
As doth eternity.

It is obvious, secondly, that in these arts the impulse to self-enhancement through the sense of creation is satisfied in a special form. For we remain aware, subconsciously as a rule, that the given object is 'really' only dead matter, stone, wood or metal, whatever it may be. But in the mere fact that the dead material has been vivified so as to express a live and organic being and that this result has been aimed at and achieved by the purpose of a human artist, whose ideas we as onlookers can repeat, there is given a sense of overcoming an obstacle and of transcending limitations. Obviously there ought to be no attempt at a complete illusion. The object must not pretend to be anything but its real material. For, if it did this successfully, the sense of overcoming would be lost.

Lastly it may be remarked that, as in the case of musical forms, some of these visual forms may in their intrinsic character convey a sense of freedom of action and movement, so that there is a reinforcement of that more general feeling of the same nature which belongs to them as art. A pattern of lines or curves may give the impression of free and unrestricted movement. We call such patterns bold and flowing and contrast them with others which appear as cramped and constricted, or wavering and undecided. Or, to take an instance from architecture, the stone structure of a Gothic cathedral, the pressures of which are in reality downward, yet conveys to us the impression of upward striving and aspiration. We may venture to say that it makes more appeal, at least to those of our own race, than a building in the more confined Classical style, which by the terminating straight lines of the roof gives a sense of closure, almost of heaviness.

(b) *The directly representational arts*

Literature, as well as painting and sculpture, may be called directly representational. It imparts to the reader or listener ideas, feelings, or a history of events through the medium of the words directly associated with them; while sculpture and painting evoke images of natural objects

through the medium of what appear as imitative copies. The products of these arts do not convey by their mere form the fact that they are creations of the artist, as do music and architecture. It is possible for words to purport to give nothing but a literal description of a slice of life. Painting and sculpture may be purely slavish reproductions of nature, as near to photographic accuracy as possible. No one would speak of aesthetic value in these cases. For such value there is necessary, as already stated, a sense of imaginative creation. This awareness may be induced in various ways, for instance through the balance and harmony of the parts, which make the work into a complete and rounded whole. We demand of a picture that it should have a certain harmony of composition, of a story or poem that it should have a form of development, 'artificial' if we like to call it so, proceeding from an opening and rounded off to a natural conclusion. It is long odds against any chance section of life having this quality. When it appears, we feel it to be the result of the work of the artist's imagination. It may be added that the term creation, as here used, must not be taken in too narrow a sense. An act of selective attention, purposively made, must be considered to fall into the same class.

Within this total form, moreover, it is possible for the artist to lay stress on certain parts or aspects. The delicate play of light is emphasized by the impressionist school, the mysterious depth of shadow by Rembrandt, while a Piero della Francesca or a Cézanne can make a given figure in the composition stand out with significant force and weight from its background. As regards literature it is obvious that the greater part of the writer's work consists in the emphasis with which ideas, valuable or interesting in themselves, which underlie the framework of his story or description, are brought to light and made impressive. All these are methods of liberation from subjection to literal reality; to give an old expression a new turn of meaning, they may be called methods of 'idealizing'. It goes without saying that the points thus emphasized should have an intrinsic value of their own. We do not want to be compelled to contemplate either the trivial or the positively unpleasant. As I have stated already, aesthetic value arises from the addition of an element of the ideal to that which already possesses some worth or excellence. On the other hand, we do not want either the impossibly perfect or anything so fantastically contrary to our recollections of the real world as to shock our preconceived notions. In all cases it is possible to seek a happy mean between ideal and verisimilitude.

In this connexion a brief reference must be made to the part played

by metre in poetry. Poetry is primarily distinguished from other forms of literary art by the fact that it is made up of words with a rhythmical pattern of sound, more or less regularly repeating itself. There follow from this three main effects. Firstly, the rhythm suggests movement, and thus it is able to yield values of the same sort as those of music. Secondly, if the kind of movement suggested is in agreement with the meaning of the words, it adds to such meaning a special reinforcement, increasing their total emotional effect. Thirdly, and this is what chiefly concerns us at the moment, the immediate effect of the repetition of a rhythmical sound pattern is to act as a sort of incantation, to soothe us into a partially dreamlike state. And as this way of using words is different from that of ordinary practical life, we are prepared to find that that also which is meant by the word sounds is neither a literal description of reality nor intended to have a practical end. We are set and prepared for the imaginary. Wordsworth has said this, speaking of the "tendency of metre to divest language in a certain degree of its reality, and thus to throw a sort of half-consciousness of unsubstantial existence over the whole composition" (5).

An objection which might be made against our theory would be that there is in every sort of art, and particularly in pictorial art, a certain value which seems to be connected with the mere fact of limitation. There is the art of the miniaturist, of the 'exquisite', of that which obtains its effects from finish in detail on a small scale. We speak of a 'rounded perfection'. The very term perfection, it might be said, implies the finished and finite, which there is no impulse to pass beyond. It can be admitted that there is such a thing as beauty of perfect proportion and balance, which may exist in a spatial pattern or in a human figure. When such an effect gives the impression of having been due to some sort of idealization by the artist, it has, as we have seen, an aesthetic value. In itself, however, the mere beauty of perfect proportion and balance does seem to give the effect of a limitation. In the case of the human figure there is a beauty of perfect symmetry, due to physical health and complete development. It can be fairly described as a physical beauty. It has its value for one who beholds it and can seem by sympathy to share in its perfect health and strength. This value can be reproduced by art. But art which does nothing more than reproduce this or any other sort of finite perfection does not possess the distinctive character of aesthetic value. That only exists when there is some sense of the transcendence of limitations, whether perhaps by showing the existence of some spiritual character behind the physical beauty, or by some kind

of purely pictorial emphasis, of which examples have been given. Art of the same kind as that of the miniature may, however, possess a certain value peculiar to itself. For minuteness and finish of detail, whether in a picture or in a literary description, may appear as a sign of the loving devotion of the artist, of the completeness with which he has given himself to his subject, resulting in an almost infinite capacity for taking pains. In the human qualities thus disclosed there has been no narrowing of the self, but rather a transcending of limitations, in which we, as onlookers, can participate.

VI. THE DISINTERESTEDNESS OF ART

The basis of the enjoyment of all forms of art is the ideal reconstruction of valuable experiences in which the artist enables others to participate. But never, at least in normal cases, do we partake in the imaginary situation as if it were real. A certain dissociation takes place. We can at the same time be participators in an emotional situation and also more or less detached spectators. This point is of especial importance in literary art. The aesthetic experience may arise from the fact that neither the author in composing, nor we in sharing his ideal experiences, are wholly carried away and absorbed by them, as we probably should be in real life. No conscious experience as such, with its affective colouring, is directly communicable from one mind to another, certainly not at least through those signs which we call words. The mere word denoting an emotion, such as 'love', 'pride' or 'hope', is only an arbitrary sign. By the use of it, even if its meaning is understood, very little of the full living reality of the experience is likely to be imparted. Even if the writer is himself intensely possessed by the emotion, he must yet draw away from it a little in order to describe it and convey it to another. Unable to do this directly, he must use indirect methods, giving, for example, some of the sensory and overt signs of the emotion, making comparisons or similes, or using other associated ideas in order to bring out his full meaning. Thus Shakespeare can say of love that it "adds a precious seeing to the eye", Swinburne that it is "first and last of all things made", the inner spirit of the world, that it cannot be bought with gold or bound with iron, that its absence would turn heaven into hell and its presence hell into heaven. All these are not the incommunicable inner core of the experience itself, but consequences and comparisons. The comparison by means of the simile is perhaps the most important of these ways of conveying and enhancing a meaning. In its

most characteristic form it consists in taking a given situation or some human quality, which arouses a certain kind of emotion, and comparing with it another situation or fact, which is usually simpler and more concretely physical but can also evoke an emotion of the same general kind. We always tend to image the psychical in a material form, and the simpler and more concrete that form, the easier is the comprehension of the psychical. Thus we return from the simpler to the more complex with an added sense of its full emotional meaning. The conjunction of the two kinds of experience enlarges and deepens both. Indeed in our minds it is possible for the two objects to be blended into one composite image, which is transfused by the strengthened emotion resulting from both of them. Thus the poet wishes to make us realize the restfulness, after times of trouble, of living under the steadfast rule of a King who "reigns in righteousness". Then he is struck by the comparison, vivid to those who know what it is to have struggled across the desert, with "the shadow of a great rock in a weary land". Examples from poetry could be multiplied.

They stood aloof, the scars remaining,
Like cliffs which had been rent asunder.
A dreary sea now flows between—
But neither heat nor frost nor thunder
Shall wholly do away, I ween,
The marks of that which once hath been.
Her eyes were deeper than the depth
Of waters stilled at even.

We come back from these comparisons, not to the emotion in its originally simple and perhaps more crude and violent form, as it possesses and absorbs one in the ordinary conduct of life. It has been transfigured by the ideal comparison, even if that comparison itself involved bringing in facts of a more material order. Enlarged by a fuller wealth of associative meaning, it remains something from which the self has once detached itself, and in which, as a total experience, it is still able to feel a certain freedom of self-enjoyment.

This partial dissociation takes a special form in tragedy. The most obvious thing to say of tragedy, from the present point of view, is that in it there is a recreation of those human values which in real life may be drawn from struggle and suffering. But this would hardly be enough. For it would seem that there are tragedies, generally held to be great works of art, in which no one of the characters shows any of the redeeming

and ennobling effects of suffering. There is a tragic grandeur in Macbeth's outburst of despair:

a tale
Told by an idiot, full of sound and fury,
Signifying nothing.

or in a poem of unrelieved pessimism, such as James Thomson's *City of Dreadful Night*. The explanation must in the main be based on the fact that, in beholding this sort of tragedy, we are not wholly absorbed through sympathy in the actions and sufferings of the characters. We are also, because of a partial dissociation, spectators of them. This does not mean that we are, as spectators, wholly passive. The mental state is likely to be somewhat complex. There may be a tinge of pity in it. But the more important element in it is rather of the same nature as the sense of the sublime. Psychologists have succeeded in analysing the sublime. It consists in an initial self-abasement before the spectacle of great power, followed, when it is seen that the power threatens no actual danger, by a sympathetic self-identification with that power and an ideal sharing of the life attributed to it, the self-abasement thus being transcended. In face of a great tragedy we may primarily seem to ourselves to be confronted by a world moved by a power which is inscrutable to human judgement and does not conform to human standards. Before such a power there is a tendency to abasement with a colouring of fear. But then it is also possible for the writer to suggest to us that the very mysteriousness of this power gives occasion for wonder and speculation, and that it may act under laws which have more justification than we can see. Thus we can, even if only vaguely, give ourselves over to it and partially identify ourselves with it, so rising above the initial abasement and fear. Such may be the meaning of the book of Job. And a similar attitude is suggested in Lear's words, at the climax of the most completely tragic of Shakespeare's plays:

And take upon us the mystery of things,
As if we were God's spies.

It is in this partial self-identification with the viewpoint of a universal mind and the consequent release from self-depression that the essence of the aesthetic value of tragedy is to be found. It would be possible to take up a similar attitude towards the tragic happenings of real life. But this is felt to be somewhat inhuman. Active pity and horror are more natural towards the real and are hardly compatible with that spirit of detachment in which the literary artist sits down to describe events and draw out their implications.

It may appear that a theory which says that aesthetic quality is something not self-subsistent but added to other valuable experiences, and that this something is a colouring of unreality, is a detraction from the dignity of art. Many think that a great poet, or a great painter or musician, is one who gives us a 'better', 'deeper' or 'truer' view of life. Those who use this language must ask themselves what is the fundamental difference between aesthetic and other experiences. Human emotions and values as directly experienced in the presence of real objects are not in themselves rightly called aesthetic. Love, the spirit of adventure, the feeling of religious awe, and that of the mystery of the universe, may be directly experienced without any idea of communication to others, and in such a case we do not ordinarily think of them as aesthetic experiences. There must be *some differentia*, one imagines, of the aesthetic experience. And to say vaguely that it is a better or deeper form of ordinary experience hardly seems satisfying. What I have been trying to show is that emotions such as these take on the aesthetic quality when communicated indirectly through words, or it may be through musical sounds, paintings or other visible structures. In such ideal forms, though losing something of their direct original intensity, they may in all cases (and particularly through verbal communication), while acquiring an additional value in the sense of freedom, also gain in width and meaningfulness and be more self-consciously realized. The artist becomes the spectator who "sees most of the game". In the attitude of partial detachment we are more free to develop and realize the emotions and their values. The philosopher and the religious teacher also try to communicate experiences, but they are, as such, limited by a definite end, that of proving to and convincing others. The poet is able to work up and embroider his theme, his essential aim being an enjoyment in freedom and the imparting of such enjoyment to others. He may sometimes be more convincing than the systematic thinker. But that is not his primary end.

VII. CONCLUSION. THE UNITY OF THE AFFECTIVE STATE

I have mentioned several elements of value as capable of forming part of the total aesthetic experience. There is first of all the intrinsic value of the experiences, in all their endless variety, which are conveyed imaginatively by art. Added to this there may be sensory pleasure, intellectual satisfaction in the comprehension of form, the feeling of creative activity and the sense of freedom in a world of ideas, together with that consequence closely bound up with it, the possibility of increased

richness of association or meaningfulness. I think it can be maintained that simultaneous mental elements of the nature of feeling, especially when they are not mutually contradictory, i.e. are all either pleasant or unpleasant, tend to blend into a unitary state. No doubt the point has been argued, and the results of experiments, which however have been mostly based on sensory material, have been variously interpreted. But I believe that on the whole the principle of the tendency to unity in the affective state maintains itself. If so, it is easy to understand how the various elements of value distinguished above naturally coalesce into a unitary whole, from which it is not easy to separate out the components. I have tried to show that in this complex whole, what is distinctive, what constitutes the *differentia* of the aesthetic state, is the sense of freedom and the consequent self-enhancement which naturally go with ideal experience.

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(Manuscript received 14 June 1936)

CORRELATIONS BETWEEN PERSONS

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I. REASONS FOR CORRELATING PERSONS RATHER THAN TESTS

INDIVIDUAL psychology has been defined as that “branch of psychology which is concerned with the mental characteristics of different persons”, and, as its definition implies, may follow two alternative lines of approach. It may begin with the more salient mental characteristics and examine how each varies among a number of different individuals—as when we plot a frequency curve for the distribution of intelligence in a sample population; or it may begin with single individuals and examine how each person varies in regard to the different characteristics—as when we plot a psychographic profile to show the uneven abilities of a backward child. With either line of inquiry, we may then proceed to compare two or more sets of variations among themselves: in the former case, we look for resemblances between the distributions of the several characteristics, grouping the superficial traits together, and ascribing their manifestations to deeper and more general faculties or factors; in the latter case, we note resemblances among the several individuals, classing them according to sex, race, social status, family inheritance, or so-called mental types.

When we aim at quantitative conciseness and exactness, the first set of generalizations—the resemblances between traits—may be expressed by correlating tests (or other quantitative estimates of the several traits); and the second set—the resemblances between individuals—may be expressed by correlating persons (or by some equivalent device).

These two complementary lines of approach have been termed by Stern⁽¹⁾ the study of inter-individual and intra-individual variability, leading to what he terms ‘horizontal correlation’ and ‘vertical correlation’ respectively.¹ Much the same contrast reappears in all descriptive sciences: thus ‘general geography’ takes particular conditions or products and studies their distribution over the face of the earth, while ‘regional geography’ takes a particular region and describes the various conditions obtaining, and the various products found, in that particular spot. It seems clear, however, that the two lines of approach should lead to consistent, and, in the end, to identical conclusions. So far as research is concerned, the choice of the one method or the other must turn partly upon the data accessible and partly on the nature of the immediate problem. In general, it may be said that, if we are adopting correlation as the best method of summarizing the data, and are seeking the most concise form of generalization possible, then the obvious procedure, where we have more tests or test items than persons, is to start by correlating persons, and, where we have more persons than tests, to start by correlating tests. It is clear that this principle will usually yield, not only the smallest number of descriptive coefficients, but also coefficients with the smallest probable errors.

The calculation of correlations, however, is by no means the only statistical device for investigating supposed sources of variation. It is the method most familiar to psychologists; but in other branches of biology other modes of analysis have come into use. Often, indeed, to calculate a complete set of correlations would be as laborious as it is artificial.

For example, in studying the psychology of sex differences, members of each sex have been measured by tests representing every cognitive level ((3), pp. 273–84). With such a problem the enthusiast for correlation would then apparently instruct the research student to correlate all the persons among themselves, and so demonstrate that one woman resembled another woman, and one man resembled another man, more than the men resembled the women. “Thousands of such correlations”, it is said, “may have to be dealt with in any major study.” But in such cases it is at once

¹ ‘Horizontal correlation’ consists in “comparing two characteristics, m and n , among a large number of individuals, $A, B, C, \dots, Z$ ”; ‘vertical correlation’ consists in “comparing two individuals, M and N , in regard to a large series of characteristics, $a, b, c, \dots, z$ ” ((1), p. 17; cf. also pp. 177, 282–3, 375).

more expeditious and more accurate to proceed by the analysis of variance and covariance ((20), p. 210), and to separate (i) the variance due to the characteristic factors that differentiate the two classes from (ii) the individual variations within one class and the other. If we desire to specify the distinguishing characteristics in terms of psychological tests, we have merely to take the ratio of the difference between the two class means for each test, and express this difference as a multiple of the standard deviation of the two groups (correcting, if necessary, for any irrelevant partial correlations between the tests), and then use this ratio to weight each test in a regression equation ((3), p. 283; (4), p. 69). The same method may be applied to diagnosing *any* psychological type. The diagnostic factor may be defined in the usual way as the component which contributes a maximum to the total variance, and the maximum can be found along familiar lines by the method of least squares (see below, pp. 76 and 81).

Occasionally the number of tests may be too large for such a procedure. With the Binet scale, for instance, there are more than 60. Here the investigator may be content to rank the various tests in order of success for persons of different types, and then correlate the boys with the girls, the defective with the normal, the poor child with the well-to-do, to show how far the contrasted groups differ. Yet even here the rank differences are more informative than the rank correlations (cf. (2), pp. 143, 194). In inquiries such as these we have an external criterion specifying to which sex or type each individual belongs; but, as we shall see in a moment, precisely the same principles may be applied when the diagnostic factor has to be deduced from the test measurements and there is no independent basis for separating the types ((13), pp. 286 f.).

In many cases, no doubt, the resemblances between persons may be of immediate interest in and for themselves; but it is of greater interest to inquire what produces such resemblances, and how they are to be classified and explained. Accordingly, in its search for explanatory principles, psychology goes further, and endeavours to trace these empirical results to certain underlying causes or laws.

The same causes and laws will ultimately be reached whichever avenue of approach we adopt, whether we start with the resemblances between persons or with those between tests. From this wider standpoint, therefore, the adoption of the one line or the other appears to be a matter of practical convenience rather than of theoretical import. Correlating persons is the exception rather than the rule; and, if we examine the various instances where personal correlations have been employed, it will, I think, usually be found that this method is adopted chiefly where the original measurements are subjective rather than objective in character.

II. EXAMPLES OF CORRELATING PERSONS

A few instances will make this clear, and at the same time demonstrate both the feasibility and the occasional value of this second line of approach. One of the commonest requirements of applied psychology

is to sum up the various characteristics of a particular person in terms of a brief list of fundamental qualities. Tests, preferably in group form, are applied to a number of individuals. Abstract qualities, capable of explaining the actual test performances, are deduced by some mode of factor analysis, that is to say, by endeavouring to discover the smallest number of independent factors which will most fully account for the variance and covariance observed; and, from the observed correlations between the tests, regression equations can then be computed whereby measurements for each person can be estimated in regard to each of the factors thus inferred. The factors themselves, of course, need have no more real existence than the lines of latitude and longitude upon a map. In many inquiries, however, it is difficult, if not impossible, to obtain satisfactory correlations between the tests because the tests are not sufficiently objective; and in such cases it is often better to begin by correlating persons. I have followed this procedure in several minor researches; and have taken it for granted, but never formally proved, that the ultimate results would be the same.

For instance, on attempting to estimate individual differences in mental imagery by means of would-be objective tests—the letter squares of Binet, the word tests of Kraepelin, the sensory tests of Netschajeff—it nearly always appears that the resulting measurements have a low reliability and may be flagrantly inconsistent. On adopting the more subjective methods—systematic introspection or a quantitative questionnaire—the results become more concordant and conclusive. But then we can no longer correlate the measurements for the various traits, visual imagery, auditory imagery, and the like, unless we are prepared to assume that each man's standard of vividness is the same as that of the others—a somewhat dubious assumption. If, however, abandoning all attempts at the absolute measurement of vividness, we merely require each man to rank his memory images for certain specified experiences in order of their vividness for him, we may legitimately correlate the orders obtained from different persons: we then find that, with one and the same set of experiences, the visualizer approximates towards one order, the audile towards another; and we can thus not only determine the so-called 'imaginal type' to which each person belongs, but also measure the degree to which he approximates to this type or to that. In an early research⁽³⁾, in which both methods were followed, it appeared that the reliability of the latter method was at least twice as great as that of the former.¹

More generally, it is, as a rule, when we possess no external or impersonal standard that we find ourselves compelled to correlate the judgements of different persons with one another, or with that of an

¹ My own conclusions were subsequently confirmed by Mr J. A. Davies, one of my research students, who investigated the imagery of over ninety children by all the commoner devices and tests ((3), pp. 251 ff.).

ideal or typical person. With cognitive tests we can usually assume that the true answer to each problem can be directly and precisely determined. When Binet asks the 11-year-old child to arrange five weights in order, he finds the 'true order' by placing the weights on a balance: (even here the child's relative amount of success might be more closely assessed by working out a correlation between his order and the true). In tests of other qualities, however—aesthetic appreciation, for instance—there may be no direct or objective means of determining the true answers or the true order. These have consequently to be estimated or deduced from the correlations between the persons themselves.

Several investigations carried out both at the Institute of Education and at University College by research students and myself appeared to confirm the value and the validity of the procedure in this field. There is, for example, no way of knowing whether *A* really gets as much pleasure as *B* from looking at Picasso's *Clown*; but *A* can certainly tell us whether he prefers Picasso's *Clown* to Watteau's. Thus, we can get a group of persons to rank a selected set of pictures or other aesthetic stimuli in order of preference, and then correlate their rankings either with each other or with an empirical or an *a priori* standard. Miss Pelling and myself obtained such rankings from children and adults of almost every age and social class. The correlations obtained from a representative sample were subjected to a rough factor analysis, the method adopted being one that I had already employed with educational capacities, except that correlations between persons instead of correlations between tests now formed the starting-point. As a result, it seemed possible to analyse aesthetic preferences or capacities in much the same way as educational capacities had been analysed, and to detect, not merely the presence of a 'general factor' (the 'ideal' or 'true' order, approximately that of the recognized expert on art) influencing all the persons tested in a varying degree, but also (at any rate with children) some hint of specific or group factors indicating the influence of sex, of emotional interests in particular subjects, and of personal or temperamental attitudes analogous to those described by Bullough in his classification of aesthetic types.¹

Individual preferences in almost every field lend themselves readily to this method, largely because they are more easily expressed by order or ranking than by measurement in absolute units. Thus it is instructive to compare children's preferences for different school subjects at different ages, and in the two sexes, and with their individual abilities in the subjects concerned ((6), p. 278). Both the preferences and the abilities suggest the operation of specific factors, producing what are popularly termed types—

¹ A brief preliminary account of the experiment will be found in (5), pp. 289 ff. Miss Bulley and I have obtained similar data on preferences for objects of industrial art. Miss J. M. Woods found the method equally satisfactory for estimating and analysing literary appreciation, and a later study by Miss E. Williams confirms many of her conclusions. Miss Wright attempted to apply the same procedure to testing musical appreciation, and Mr Wing has since worked out a series of ingenious tests for this purpose. Miss Dewar has followed up the investigation on the appreciation of pictures with much the same results. The more recent inquiries, avowedly tentative, are accessible in unpublished degree theses.

literary or verbal, manual or practical, scientific or mathematical, and the like ((4), pp. 58-9).¹ More recently, Beebe-Center has used the same method to study preferences for smells, working out correlations between persons and applying Spearman's method of factor analysis to the results (15).

Even in the sphere of sensory discrimination, as Mr Binns and I endeavoured to show in an earlier research, the method may at times be of value. For example, in order to test efficiency in examining woollen fabrics by touch, wool-sorters were asked to arrange selected fabrics in order of heaviness, texture, and the like. The tactile discrimination of each testee was then estimated by correlating his order with that of a weighted average furnished by all testees, the most expert being weighted the most heavily. For a few qualities the true order could be independently ascertained by physical measurement—weighing the material to determine its heaviness, counting the threads to determine its texture, and so on: these cases, therefore, provided an instructive check on the accuracy of the statistical deductions ((9), p. 93).

It is perhaps most of all where we are investigating a complex pattern of characteristics that the method is especially appropriate. Individual psychology, for example, often inquires whether persons, who resemble one another physically, either for physiological or for biological reasons, also exhibit mental resemblances, and if so in what degree. Accordingly, in comparing parent and child, or brothers and sisters, in order to determine whether they show general similarities such as might be ascribed to inheritance, the most natural method is to measure either member of the pair for as many mental traits as possible (all traits being expressed in comparable units), and then to correlate the two individuals—the method recommended, for example, by Stern ((1), p. 375). Actually, in nearly all researches on mental heredity in the past, instead of measuring a single pair for many traits, most investigators have preferred to measure many pairs for a single trait, or at most for two or three. It would be

¹ Girls, as I have shown, tend, in their preferences, as in their aptitudes and attainments, to show a verbal and literary rather than a scientific or mathematical bias ((6), p. 278). But here again the rank differences are far more informative than the mere correlations (cf. *supra*, p. 61); and, when it comes to assigning particular individuals to this or that 'type'—distinguishing the 'non-verbal' child from the child with arithmetical disabilities and both from those who are dull or defective all round—a diagram (the so-called 'psychogram' or 'mental profile') seems in practice the most instructive method of all ((4), p. 64, fig. 9, i-iv). To see how far aptitude and preference go hand in hand for the same school subjects, the method of correlating persons has been far more frequently used than that of correlating tests or subjects: but here, strangely enough, it has led to sharp contradictions—Thorndike finding, in most cases, a high correlation (7) and his critics an extremely low one (8).

worthless to calculate correlations between persons with two or three traits only; so the investigator usually calculates the correlations between the several trait-measurements—the trait being taken first as found in one set of relatives (e.g. all the parents) and then as found in the other set (e.g. all the children).¹ If, however, we are seeking to compare relatives in regard to traits that are temperamental rather than intellectual, the latter method proves distinctly unreliable, and we are thrown back on the former. Indeed it is more especially in the qualitative field, where interest turns on relative rather than on absolute amounts, that the device of correlating persons will prove most useful.

In a study of "the general and specific factors underlying the primary emotions" (10), I found, on applying factor analysis to assessments of emotional behaviour, indications of at least two specific factors, which seemed to account for the familiar dichotomy into extraverted and introverted types. These, in their pure or abstract form, appeared to be characterized by an opposite order of strength for the several emotions. Thus, a person's hypothetical measurement for the factor of 'inhibition'—virtually a 'saturation coefficient' expressing the correlation between his order and the ideal order furnished by the factor analysis—expresses his tendency towards inhibitive emotions or the reverse. From this a practical corollary of some importance seemed to follow. Teachers, in their reports on a case of delinquency, neurosis, or vocational guidance, are repeatedly asked to rate the child for a number of emotional proclivities. But each observer has his own standard of what constitutes average or abnormal behaviour; hence his efforts at absolute assessment may have little value. But every one can say whether a particular child is more prone to anger than to fear, to joy than to grief, to self-display than to self-suppression, and so rank the more obvious emotions according to the order of predominance in the person under review. When inquiring into practicable methods of temperamental assessment for purposes of vocational guidance, I put this method to the test. Here what were asked for and correlated were "not assessments for the same character quality among a group of different individuals, but assessments for a set of different character qualities in the same examinee". It was found that, under certain conditions, the reliability of this alternative procedure might be much higher than that of the more usual method ((11), pp. 65 f.). For clinical work it has proved, at any rate in my own experience, remarkably helpful. Most people find it far easier to think of a person's character as consisting in a pattern of tendencies differing in relative strength rather than in a sum of isolated traits to be assessed on a normal curve; and the pattern can be expressed either as a 'temperamental profile' or (for quantitative research) as a 'coefficient of saturation'—i.e. a correlation between the person's assessments and what I have called a 'key subject or standard personality'. For the diagnosis of neurotic tendencies and the like, this character pattern is often much more suggestive than any absolute assessments, especially as the child's general level of emotionality can be sufficiently gauged by the psychological examiner himself.

¹ For the difficulties involved in applying the usual method of correlation to such problems, and for an alternative procedure, see (20), pp. 214 f.

Finally, the reliability coefficient, when calculated for human judgments instead of for the results of objective tests, is itself an example of correlating persons ((31), p. 189 and references); and may be used as an indication of those persons' capacities for making the judgements required. When, for example, a group of examiners mark a set of candidates' essays, the intercorrelations between their mark lists may throw considerable light upon the value of the examination⁽¹²⁾. The correlations may then be subjected to factor analysis in order to discover common, partly common, and purely personal influences¹; and, where the subject permits of no clear objective assessment (e.g. literary subjects as distinct from mathematical), the correlation of each examiner with the weighted averages obtained by combining all the lists may be taken as a measure of his efficiency⁽¹³⁾.

These, then, are the types of investigation to which the procedure I have described has seemed especially applicable. Two questions, however, arise. First, what particular conditions must be observed, and what modified procedure employed, if this alternative approach is to possess a reasonable validity? Secondly, what relations, if any, obtain between the estimates derived by correlating persons and those that could be derived, at any rate in certain cases, by the commoner device of correlating tests? To both these questions opposite answers have recently been given. Hence, it appears an opportune moment to examine the method a little more explicitly, and to justify the applications that have already been made.

III. VALIDITY OF THE METHOD

The question of validity has recently been raised in connexion with the problem last mentioned—the investigation of examiners' marks. When it is desired to estimate the efficiency of a new mental test, the usual practice is to seek an external and independent criterion—some old and well-tried test the results of which are unquestioned, or the judgement of some trained and practised observer who has become thoroughly acquainted with the examinees during a long period of years—and then to correlate the marks or measurements furnished by the new method with this accepted standard. Suppose, however, that no such independent check is procurable. Then it becomes necessary to extract the best possible criterion from the actual figures in front of us. We may take either a plain average as a fair approximation, or, better still, a weighted

¹ An important aspect of 'reliability', amenable to the same device, is the stability of the relative difficulty of test-questions, as shown by the answer-pattern: cf. (2), Table V, pp. 137 and 194.

average—the weights being determined by the hypothetical correlations with the general factor assumed to pervade and influence the whole set. This plan is regularly recommended in standardizing a new set of objective tests; and it seemed an obvious proposal in considering how to gauge the value of subjective judgements.

But, on putting forward this suggestion¹ as a practicable means of analysing data collected from various examining boards, I found very different opinions expressed as to its legitimacy. One of my colleagues strongly criticized the extension of correlation to problems of this type; another believed that the method might be developed into a fruitful technique capable of many further applications.² The criticisms, I fancy, can be met by acknowledging that in general the method can only be validly employed when certain conditions have been observed, and by modifying it in one or two minor respects. As a result, no doubt, the applicability of the method will become seriously restricted. But I hope to show that the same questions may often be answered, and, indeed, often have been answered, by the older procedure of correlating tests or traits.

Let us note, however, at the outset, that the convenient antithesis between test and person is narrow, inexact, and at times misleading. As we have already seen, what appears as a test in one investigation may be treated as a person in another, and vice versa. Outside psychology analogous problems have recently been attacked by statistical methods—e.g. in the determination of genotypes and phenotypes among animals and plants, and of their diagnostic characters; but in such researches, more often than not, the two sets of variables to be compared are neither traits

¹ I had used such correlations, not only for evaluating marks of teachers and examiners for elementary school subjects and factorizing the results ((4), pp. 51 ff.), but also for studying the results of University examinations with which I had been connected.

² Dr Rhodes' criticisms have mainly been expressed in private correspondence; he has, however, outlined his own suggestions for attacking the problem in a recent memorandum ((13), Memorandum II, pp. 315 ff.), and has shown elsewhere(14) that his suggestions are capable of a more general application. Dr Stephenson, on the other hand, has just published two instructive papers warmly advocating methods such as those described in my memorandum. In his first experiments on the subject(16), he deals with aesthetic appreciation on lines similar to my own, asking various persons to judge or arrange in order of merit a series of aesthetic stimuli; on working out the correlations between the several persons, he reaches results closely resembling those I had myself obtained. In a later article in this *Journal*(17) he has endeavoured to meet the difficulties pointed out by Godfrey Thomson(37) and to improve upon the technique used by Beebe-Center(15) and others; he also describes formulae for use in the factor analysis. These appear to be the same as those already given in my memorandum for correlations between 'persons' when the persons' judgements are analogous to 'tests'; for more general purposes I should regard them (for reasons there given) as not altogether valid and as furnishing an approximate analysis only.

nor individuals, but inanimate conditions—differences of soil and of manurial treatment, of weather, season, time, and the like. The real antithesis is between comparing rows and comparing columns from one and the same matrix; and, at bottom, the two modes of approach are merely alternative ways of simplifying an identical set of figures by a succession of linear transformations.

If, then, we choose the method of correlation for such purposes, it should be remembered that we can safely correlate *persons* only when the traits or tests fulfil much the same conditions that we should require of the persons when correlating *tests*: that is to say, as a general rule: (i) the tests must be sufficiently numerous to keep the probable errors low; (ii) they must form a representative or random sample of the total population of tests; (iii) the marks or measurements obtained from different tests must all be reducible to terms of the same unit; (iv) the distribution of the measurements should be, as nearly as possible, normal; and (v) the correlation should be linear. Otherwise a correlation between persons is likely to be meaningless.

Yet, even when the method of correlation is artificial or invalid, we may, however small the sample, and whatever the form of distribution of the variables, still attempt an analysis of variance and covariance (⁽²⁰⁾, pp. 160, 198). In such cases the variances and covariances will primarily be constants descriptive of the particular sample studied: the usual tests for significance may at times be tried; but strictly they will be applicable only when the distribution of the dependent variable (e.g. of the traits or tests if we are correlating persons) is normal. The essential problem is to resolve a set of variables into their components; and, as I hope to show, this can be done, in psychology as in other sciences, without assuming the special presuppositions which correlation as ordinarily interpreted seems essentially to imply. Accordingly, as stated at the outset of my memorandum (13, p. 247), it seems far preferable, particularly when the relations between persons are to be studied, to start with covariances or product sums rather than with correlation coefficients.¹

¹ Since in my memorandum the 'tests' were candidates' scripts, and these may be supposed to have approximately a normal distribution, most of the final results could there be given in terms of the more familiar correlation coefficient. It is of special interest to note that, since my memorandum was written, Kelley, in his latest method of factor analysis, has also decided to start from covariances rather than from coefficients of correlations ((33), pp. 1, 5). Kelley, however, does not make clear that the factorization of covariances will in general produce somewhat different results from the factorization of correlations. Thus he states (*loc. cit.* p. 1) that "the outcome" of his procedure is "identical with that given by Hotelling" (32): this, as I have shown elsewhere (38), is true only when the standard deviations of the observed measurements are equal.

Covariance, like variance, may be treated as the sum of separate contributions: the mental measurements involved combine like vector quantities or (since a matrix is a series of vectors) like the products of matrix multiplication. Now, the general principle underlying most applications of matrix calculus is the separation of variables from constants by means of such matrix products; and, in all the problems of factor analysis in psychology, the fundamental issue may, in my view, be conceived as one of linear dependence or independence—a far broader concept than correlation. Hence, the most convenient mathematical treatment appears to be an application of matrix algebra, supplemented by the theory of probability, on lines analogous to the use of linear operators in modern physics. In physics any assembly of systems in a definite state is represented by a vector; variables are expressed by linear operators; and the divergence between two vectors indicates the probability of a transition from one state to the other. If, in later research, we require to remove the restriction that the relations are to be described by linear equations only, we can still follow the physicist and employ tensors of a higher order than the first. The advantage of this mode of calculus would be that, in psychology as elsewhere, all equations could be presented in a form which is independent of the choice not only of a unit but even of a 'measure code'. Thus, one objection raised by those who criticize the proposal to correlate persons would be obviated—namely, the apparent incommensurability of the original units used for the tests.

To grasp the elementary principles of matrix algebra, nothing more advanced is needed in the way of prior mathematical equipment than the schoolboy's knowledge of the use of determinants for the solution of simultaneous linear equations. In the references on p. 95 I have given a list of books which will be most helpful to the student who wishes to carry his knowledge further ((21)–(34), and particularly (25) and (26)). It is unfortunate that many of the matrix theorems needed in statistical psychology are scattered over a number of different books, and that in the best known (e.g. those of Hilton or Bôcher(21)) the exposition is confined almost exclusively to square matrices. Thurstone's recent volume on the *Vectors of the Mind*(34) gives a clear introduction, but suffers from the brevity with which he treats the problems of linear dependence and rank, out of which most of the difficulties arise. The first psychologist to propose that, for purposes of factor analysis, mental measurements should be treated as vector quantities was apparently Maxwell Garnett(30); but the few who have followed up this suggestion seem to have regarded the vector rather in the geometrical than in the dynamical sense. The reader already familiar with modern physics will find it instructive to note how many of the analogues of the equations required below recur in the theory of linear operators as used for quantum theory (cf. the first three chapters of Temple's concise summary(27), and for a fuller account (28) or (29)). The main difference between the vector analysis employed by the physicist and that which seems required in psychology is that in the former many of the fundamental transformations are represented by complex numbers; here symmetrical matrices take the place of Hermitian. In the sequel, however, I shall show that the introduction of complex numbers may at times be suggestive in psychology as well. What I have described as a combination of matrix algebra with probability is outlined by Sheppard ((23), pp. 92–114), who in turn acknowledges his indebtedness to Eddington; but Sheppard's terminology and notation, both largely peculiar to himself, deprive the statistical chapter of his little book of much value that it might otherwise have had for the student of psychology.

In my own memorandum I have endeavoured to give a general outline of a modified form of factor analysis that seems applicable to correlations between persons, and to deduce simple equations and formulae specially for this purpose. At the same time I have sought incidentally to show that the different methods of factor analysis already proposed—those of Spearman, Hotelling, Thurstone and Kelley—are in their essence merely special applications of the same general principles, and can be regarded (in spite of the strong expressions of mutual disagreement which their authors have at times professed) merely as alternative means of approximating to the same ultimate result. The essential difference seems to be that Spearman and Thurstone (finding a centre of gravity) are content to use first moments; Hotelling and Kelley (preferring to use a momental ellipsoid) are in effect working with second moments. As we shall see later on, if we employ higher moments still, we may reach, not indeed a more exact solution, but one equally exact with greater speed ((13), p. 287).

IV. CONSISTENCY BETWEEN THE RESULTS OF CORRELATING PERSONS AND TESTS: FORMAL PROOFS

In this paper one of my chief aims will be to justify my assumption that the factors obtained by correlating persons are identical with those obtained by correlating tests. Were it only a postulate, this seems reasonable: *factores non sunt multiplicandi praefer necessitatem*. But the mutual consistency is a natural result of the methods of factor analysis themselves, when those methods are put in a form which is adequate to the problem; and there are numerous analogues in the corresponding transformations of analytic geometry, from which all factor analysis is implicitly derived.

Factors are axes or planes of reference; and an equation like

$$l_1 p_1 + l_2 p_2 + l_3 p_3 + l_4 p_4 = 0$$

may be regarded as the equation of a point in homogeneous plane co-ordinates or the equation of a plane in homogeneous point co-ordinates. Whether there are n dimensions or only two, these alternative interpretations are always equally possible. Let me put the simplest conceivable case in non-mathematical terms: suppose Mahomet is transported from Mecca to Mount Popocatepetl at the rate of 300 miles an hour; then an earthly disciple would doubtless attribute the motion to Mahomet, while an observer on the sun would probably say that Mahomet had remained where he was and the mountain had moved to Mahomet. Both observers, let us imagine, now undertake a factor analysis to decide in what particular dimension the motion had occurred; then, whether the place is taken as fixed and Mahomet's position referred to the mountain, or whether the person is taken as fixed and the mountain referred to Mahomet, each would eventually conclude that the direction of motion was along a line running due east and west. And what holds of the option between two initial points of reference would also hold with an option between two initial frames of reference. Or, to turn to a less fanciful analogy: in physics it constantly occurs that

a composite system in a definite state can be represented either by a vector α in space A or by a vector β in space B ; a relation is then established between the two spaces, in terms of a 'product space', obtained by multiplying either A by B or B by A . Similarly, whether we treat our tests as *radii vectores* and our testees as points, or whether we treat our persons as *radii vectores* and our tests as points, the orthogonal frame of reference to which the system is ultimately referred should be identical. It is something more than a play upon technical terms to say that, in psychological statistics as elsewhere, covariance implies invariance.¹

In the memorandum just cited, I briefly intimated the chief point on which a theoretical proof depends. But, as one writer at least has questioned the existence of any relation between the two modes of factor analysis, I shall here endeavour to set out the proofs in full. The student who feels unable to follow the algebraic argument will, I hope, have no difficulty in tracing the underlying principle if he turns to the simple example worked out on pp. 90-4; and even other readers may find a concrete demonstration more convincing than an abstract mathematical deduction.

$$\text{Let} \quad \begin{bmatrix} m_{11} & m_{12} & \dots & m_{1p} \\ \dots & \dots & \dots & \dots \\ m_{t1} & m_{t2} & \dots & m_{tp} \end{bmatrix} \equiv {}_tM^p$$

be a matrix of real numbers (e.g. test measurements) having p columns and t rows.² For simplicity let us say that p persons have been tested for t empirical traits. In correlating tests the first step will be to convert the raw measurements into deviations about the test means. We may assume that this has already been done. There will be no need to

¹ As Prof. M. Greenwood, who has been good enough to read the following section of my paper, points out, there is here an analogy with the well-known 'principle of duality' in the study of conics and conicoids: in transformations by reciprocal polars, to every theorem about points there corresponds a theorem about lines (or planes), and vice versa: equations in Cartesian or trilinear co-ordinates expressing theorems about points (or about lines and other primes) will, if interpreted as equations in tangential co-ordinates, yield corresponding theorems about lines and other primes (or about points), and vice versa ((22), pp. 75-6). Indeed, owing to the way in which poles may be determined by the latent roots of a matrix—as in that particular form of transformation known as a collineation (cf. (24), pp. 92, 282, 293 f., and (26), iii, p. 334, ex. iii)—the parallel is something more than an analogy, at any rate so far as relations between the test measurements and the factor measurements are concerned (see below, p. 78). But Prof. Greenwood suggestively observes that the *limits* of reciprocation may throw light on the limits to the value of correlating persons.

² In the following proofs small letters with double suffixes will generally be used for elements in matrices. The matrices themselves will be denoted either by the corresponding capital letter or by placing the typical element in square brackets. A lower prefix and an upper suffix (which, of course, is not to be taken as an exponent), as in ${}_tM^p$, will indicate the number of rows and columns respectively of which the matrix is composed—points of primary importance in carrying out a matrix multiplication.

reduce the measurements still further, and express them in standard measure. Indeed, both algebra and arithmetic are simpler, and likely to be more accurate, if, when 'correlating' (or rather 'covariating', if I may introduce such a word), we divide neither by the number of items correlated nor by their standard deviations: the essential problem, as we have seen, is to analyse the variance and covariance, and this can best be done by keeping to product sums.

When we come to correlate persons we shall require to make a similar adjustment. Hence, to simplify the double analysis, I propose in this preliminary paper to assume that the measurements are also expressed as deviations about the means for persons. If we happen to have tested a population which is fairly homogeneous as regards the 'general factor' underlying all the tests (as we might, if we desired to study specific factors more especially), the means for the several persons will be approximately equal from the outset. If they are not, we can average the columns and express the figures as deviations about the means for the several persons. When these requirements are not imposed, the argument becomes more complex, but the essential principle remains the same.

(1) *Particular solution*

The methods of factor analysis current in psychology usually begin with two postulates:

(i) The first assumes each observed measurement to consist of a product sum, i.e. a weighted sum of certain hypothetical factor measurements. Thus, Spearman's initial equation ((31), p. 75) is, for his 'two factor theory',

$$m_{kx} = r_{kg} \cdot g_x + r_{ks} \cdot s_{kx}, \quad \dots\dots(1)$$

where m_{kx} is the measurement of person x in test k , g_x and s_{kx} are his hypothetical measurements for the general and specific factors g and s , and r_{kg} and r_{ks} the loadings or 'saturation coefficients' of the k th test for those factors ($x=1, 2, \dots, p$; $k=1, 2, \dots, t$).

Since this approach is more familiar to psychologists,¹ I shall follow it to begin with, and shall endeavour to give my arguments first of all in a form which may be intelligible even to those who are unfamiliar with

¹ The same starting point is taken by Godfrey Thomson ((35), p. 242, equations (1) and (2), and, in slightly different notation, (36), p. 41, equation (10)). It provides Thurstone's initial equation ((34), p. 52, equation (1)), and Hotelling's ((32), p. 418, equation (3)). A fuller account of the initial assumptions is given in my *Memorandum* (13). Since it is desirable that all writers should adopt the same notation, I have, so far as possible, changed my own to that of Thurstone (34), whose volume appeared after my notes were drafted.

matrix algebra: the few new terms and rules will be explained in footnotes. A more rigid proof will be attempted later.

As we do not desire to limit the number of factors (*f* say) to two, we shall have *f* elements in the postulated vectors instead of 2. We may accordingly express them more succinctly in matrix notation by writing

F for the *t* row vectors [$r_{kg}, \dots, r_{ks}$], and *P* for the *p* column vectors $\begin{bmatrix} g_x \\ \vdots \\ s_x \end{bmatrix}$.

Thus *P* (factor measurements) and *F* (factor loadings) will be rectangular matrices having the form ${}_iP^p$ and ${}_tF^f$ respectively; when requisite, a lower suffix, *t* or *p*, can be attached on the right to distinguish matrices obtained by correlating tests from those obtained by correlating persons respectively. Accordingly, we can now write equation (1) in the form of a matrix multiplication:¹

$$M = F {}_tP_t, \quad \dots\dots(2)$$

and, when we start by correlating persons, we shall postulate a similar equation:

$$M' = F_p P_p, \quad \dots\dots(3)$$

where *M'* denotes the transpose² of *M*, *F_t* and *F_p* the factor loadings for tests and persons respectively, and *P_t* and *P_p* the factor measurements for persons and for tests obtained by correlating tests and persons.

In the same way, the usual regression equation for "weighting the measurements so as to make" (in Spearman's phrase) "the best team of tests", namely,

$$g_x = w_1 m_{1x} + w_2 m_{2x} + \dots + w_t m_{tx}$$

((31), p. xviii, equation (25)), may be generalized so as to apply to all the factors, and written

$$P_t = W_t M. \quad \dots\dots(4)$$

On correlating persons instead of tests, we shall require an analogous regression equation

$$P_p = W_p M'. \quad \dots\dots(5)$$

¹ To multiply two matrices, it will be seen, we form a product sum by multiplying corresponding elements from rows and columns taken in pairs—the procedure every student follows when he multiplies corresponding deviates to obtain a product-moment correlation.

² The matrix obtained by rewriting the original matrix *M* with its (horizontal) rows arranged as (vertical) columns, and therefore its columns as rows, is called the 'conjugate' ((26), i, p. 5) or (better) the 'transpose' ((25), p. 32) of *M*, or sometimes (with a reference to the 'principle of duality') its 'dual' ((28), p. 13): but terms like 'conjugate' and 'dual' are best kept for cases where complex numbers are involved. It is usually denoted by affixing an accent to the symbol for the original matrix. When geometrical interpretations are implied it would be convenient if matrices were written so that their columns denoted points and their rows denoted primes.

(ii) The second postulate is that the factors, unlike the persons and the tests, shall be uncorrelated, that is, both linearly and statistically independent. Confining ourselves for the moment to the results obtained by correlating tests, we may express this by writing

$$P_1 P_1' = V, \quad \dots\dots(6)$$

where V is a diagonal or quasi-scalar matrix, i.e. a square matrix in which every element except those in the leading diagonal are zero: those in the leading diagonal will obviously contain the variances (unaveraged) for P_1 (where the subscript 1 is added to indicate that the factor measurements have not yet been reduced to standard measure as Spearman's formula requires). The relation between P_1 and M may be expressed by writing

$$M = L P_1, \quad \dots\dots(7)$$

where, if we adopt a geometrical interpretation, L will represent the orthogonal or semi-orthogonal¹ matrix of direction cosines for transforming the measurements from one set of rectangular axes to another.

It will, however, be convenient, and will involve no relevant loss of generality, if we further assume that the factor measurements have been 'normalized' (25, p. 39) and are consequently in 'unitary standard measure'.² Accordingly, by putting

$$P = V^{-\frac{1}{2}} P_1, \quad \dots\dots(8)$$

we may modify (6) to

$$P P' = V^{-\frac{1}{2}} P_1 P_1' V^{-\frac{1}{2}} = I, \quad \dots\dots(9)$$

where I denotes the unit matrix.³ In the language of vector analysis, P may be described as the 'standard basis' in terms of which other vectors are to be expressed (cf. (27), p. 8; (29), p. 49).

¹ If L is an orthogonal matrix, then its transpose L' is identical with its inverse L^{-1} (see p. 78, footnote 2). The equation $L'L = I$ is thus the matrix version of the familiar equations giving the relations between the direction cosines of an 'orthogonal transformation' or rotation of axes. A semi-orthogonal matrix may be regarded as derived from a square orthogonal matrix by deleting certain rows or columns.

² By 'unitary standard measure' I mean a scale so chosen that the sum of the squares of the measurements (the unaveraged variance) is equal to unity. This is equivalent to taking, not σ (the standard deviation) as the unit, but $\sigma \sqrt{t}$ (where t is the number of measurements). This, as I have pointed out ((13), p. 272), is a far more convenient unit for matrix manipulation.

³ The unit matrix is a quasi-scalar matrix with a unit for each element in the principal diagonal and the remaining elements all zero. On multiplying a second matrix by the unit matrix, the second matrix is left unchanged. If all our values are to be real, the above condition (7), together with the fact that the measurements are deviations about their own means, imposes the relation $f \leq (p-1)$ which may become relevant later on.

With P thus standardized we reach a new interpretation for the factor loadings F . On correlating M and P we obtain the matrix

$$[r_{mp}] = MP' = FPP' = F. \quad \dots(10)$$

Thus, when both are in standard measure, the correlation between the testees' measurements in any test j (say) and their measurements in terms of any hypothetical factor i (say) is given by the factor loading for that test, namely, f_{ij} . This is simply a generalization of Spearman's alternative definitions for r_{kj} and r_{ks} , the saturation coefficients of his two factors. By combining equations (10), (7), (8) and (6) we obtain the relation between F and L : namely,

$$F = MP' = LP_1 P_1' V^{-\frac{1}{2}} = LV^{\frac{1}{2}}. \quad \dots(11)$$

And from this in turn we obtain the important result

$$F'F = V^{\frac{1}{2}}L'LV^{\frac{1}{2}} = V. \quad \dots(12)$$

Finally, it will be seen, we can express the test measurements as a product of three matrices—two semi-orthogonal and one diagonal; for, if we begin by correlating tests, we have

$$M = F_t P_t = L_t V_t^{\frac{1}{2}} P_t, \quad \dots(13)$$

and, if we begin by correlating persons, we have

$$M' = F_p P_p = L_p V_p^{\frac{1}{2}} P_p. \quad \dots(14)$$

Our first problem, therefore, must be to solve equations (1) to (9) and find V , L , W and P for tests and persons; and our second and crucial problem will be to determine the relations between V_t , L_t and P_t on the one hand, and V_p , L_p and P_p on the other.

The form of equation (9) suggests that we begin by multiplying the matrix of observed measurements M by its transpose M' , and then seek some method of transformation which may reduce the product to terms of a 'canonical' or quasi-scalar form, V , in which all the elements except those in the principal diagonal are zero. If we post-multiply the adjusted matrix by its transpose, we have at once a table of (unaveraged¹) variances and covariances for the *tests*, or (if the original measurements are expressed, not merely as deviations about the test means but also in unitary standard measure) of ordinary correlations in product-moment terms. If we pre-multiply, we obtain a table of variances and covariances for *persons*. Accordingly, taking R to denote the matrix of variances and covariances, we may write

$$R_t = MM', \quad \dots(15)$$

$$R_p = M'M. \quad \dots(16)$$

¹ I may leave this qualification to be understood whenever I speak of variance or covariance.

A. *Correlating tests*

Let us attempt first to factorize the correlations or covariances between the tests.

If f is the number of hypothetical factors, then f will indicate the rank¹ of R . When R is a matrix of *observed* covariances, the figures must depend in part upon chance errors due to sampling; hence the rank of such a matrix will almost inevitably be equal to its order, t . But if we take R to represent the matrix of *true* covariances (as is assumed by Hotelling⁽³²⁾, p. 422) it may be feasible, and, indeed, it will usually be desirable, to find a far smaller number of significant factors yielding a reasonable fit. Accordingly, we must envisage the possibility that $f < t$. From this most of the difficulties in the proofs arise. Here I shall assume that the influence both of the errors of measurement and of the specific factors peculiar to one test is so small as to be negligible.

By equations (15), (2) and (9) we have

$$R = MM' = FPP'F' = FF'. \quad \dots\dots(17)$$

But, since R and FF' are both symmetrical matrices, this yields only $\frac{1}{2}t(t+1)$ different equations to determine the matrix F which may contain as many as t^2 quantities. For the psychologist that may seem an advantage rather than a drawback: for he can, if he wishes, define his factors on extrinsic or objective grounds, or fix them by assuming that one or more of his tests give virtually perfect reference values. Personally I am doubtful of the existence of objective factors; but how to reconcile the various interpretations of the term 'factor' is a problem I shall discuss more fully later on. Meanwhile, I shall assume that the factors may be best defined in terms of the analysis of variance, i.e. that the values required are those that will enable us to discriminate between the members of the population as widely as possible in terms of the smallest number of factors. This means that V must be so chosen that

¹ As is shown in elementary text-books of algebra, if the corresponding elements of two rows (or columns) of a determinant are proportional throughout, that determinant vanishes. It follows that, if the rows of a matrix can be expressed linearly in terms of f rows, then all the $(f+1)$ -rowed minors (and consequently all its larger determinants) vanish; in that case, assuming that at least one f -rowed minor does not vanish, the matrix is said to be of rank f . Conversely, if f is the rank of a given matrix, then it is possible to select from it f (but not more than f) rows or columns that are linearly independent ((26), i, p. 276). Accordingly, if f is the rank of R , then f will be the number of columns in F , i.e. the number of independent 'factors' that must be postulated to reconstruct R by means of equation (17). It will be observed that, in Spearman's test for a 'hierarchy', the so-called 'tetrad-differences' are really the 2-rowed minor determinants of the correlation matrix. Hence his well-known criterion ((31), p. iii, equation (5)) is in effect a special application of the foregoing principle to prove that the rank of a correlation matrix is 1.

$v_1 > v_2 > \dots > v_j$: v_1 giving the largest attainable variance, v_2 the next, ... and v_j the least. At each stage in the analysis, therefore, we shall seek to maximize the variance produced by the multiple regression (the 'variance between arrays') and to minimize the residual variance (the 'variance within arrays'). The principle leads to results similar to those obtained by Hotelling, but bases the choice of method upon something more than the mere requirement of some 'orderly procedure' or mere 'analogies' with the fitting of a curve ((32), p. 420).

I shall briefly recapitulate the general argument, partly to explain the computations that will need to be made, and partly to show how this modified statement of the problem somewhat simplifies the process of computation itself.

By a well-known theorem in matrix algebra, if R is a real symmetric matrix, an orthogonal or semi-orthogonal matrix L can always be found which will transform R into a diagonal canonical matrix V of real elements, λ_i , so that

$$mRm' = \mu V\mu' = \lambda_1 \mu_1^2 + \lambda_2 \mu_2^2 + \dots + \lambda_j \mu_j^2,$$

where m is any row-vector in M , $\mu' = Lm'$, $L'L = I$, and $V = L'RL$. The values of λ are uniquely determined by the matrix R , and will be the maximum, proxime maximum, ... and minimum values of the quadratic form, as obtained under the conditions imposed ((24), pp. 106, 170). They are therefore termed the 'proper (*eigen*) values' or 'characteristic numbers' of the matrix R ; and for factor analysis in psychology, as for quantum mechanics¹ ((29), p. 35), the determination of such 'characteristic' values forms the chief initial problem.

The reader, familiar with plane or solid geometry, will observe that the above procedure and the equations that follow are in effect a generalization to t dimensions of the regular method for determining the directions of the principal axes of an ellipse or ellipsoid—i.e. its longest, next longest, and shortest main diameters—by the aid of a discriminating quadratic or cubic (cf. (22), pp. 116 f.). Yule's treatment of the normal frequency surface for two correlated variables as derived from the frequency surface for two uncorrelated variables by rotating it with respect to the axes of measurement ((18), p. 321) illustrates the simplest possible application of the whole procedure: his equation (8) is the matrix equation $P_1 = L'M$ written out explicitly for $f=t=2$, and his equation (10) is virtually our equation (20) for the same simple case.

¹ Similar problems arise in ordinary mechanics when determining the principal axes of a momental ellipsoid ((22), p. 235) and in *Quantenmechanik* when determining a basis for a dynamical system with a *finite* number of states ((27), p. 12, (28), pp. 11, 21, (29), p. 34). Were we to take chance errors into account, we should have to deal with the possibility of an *infinite* number of factors: I suggest that here the solution may be reached along the same lines as those adopted in physics for dealing with a dynamical system with an infinite number of states ((28), p. 39 and references; (29), pp. 36 f.).

The conditions imposed involve the consistency of t homogeneous linear equations,¹ $Rm' = \lambda m'$. Hence the values can be found by solving the determinantal equation

$$|R - \lambda I| = 0. \quad \dots (18)$$

On expanding the determinant in powers of λ , the 'characteristic equation' may be written in the form of a polynomial equation in λ of the t th degree

$$(-1)^t \{ \lambda^t - s_1 \lambda^{t-1} + s_2 \lambda^{t-2} - \dots + (-1)^t s_t \} = 0, \quad \dots (19)$$

where s_i is the sum of the diagonal minors of order i in $|R|$ and s_t the determinant $|R|$ itself. $\lambda_1, \lambda_2, \dots$ are accordingly known in matrix algebra as the 'latent roots' of the matrix R . If $|R| = 0$, at least one root will be zero.

As is shown by the form of equation (19), the maximum number of non-zero roots will be f , the rank of r . The sum of these roots,

$$\lambda_1 + \lambda_2 + \dots + \lambda_f = s_1 = r_{11} + r_{22} + \dots + r_{tt}, \quad \dots (20)$$

is conveniently termed the 'trace' of R : and this in turn is the sum of elements in the leading diagonal in the product of MM' , i.e. the grand total of all the variances, $\sum_p \sum_t m_{ij}^2$. Hence we may regard $\lambda_1, \lambda_2, \dots, \lambda_f$ as indicating the respective variances contributed by the f factors, and arrange these factors in order according to the amount contributed.

It is of interest to compare this result, as reached by analysing covariances, with that obtained when the covariances can be identified with ordinary correlations. In such a case the initial measurements are assumed to be continuous variables, normally distributed, and expressed in standard measure. By the theory of multiple normal correlation, as deduced from the principle of least squares ((19), p. 296), we then have error ellipsoids (surfaces of uniform frequency or equal probability) defined by the quadratic form $mR^{-1}m'$. The usual deduction explicitly requires that $R \neq 0$ and therefore $f < t$. In accordance with the principle of duality, it may be noted (cf. p. 71), R^{-1} gives the same ellipsoid by a tangential equation as R by a point equation: with a point equation it gives the reciprocal of the latter. Since R^{-1} is the inverse²

¹ The equations $Rm' - \lambda m' = 0$ when solved for m' give the poles of R ; and, in view of the correspondence discussed in the text, it may be noted that, in the simplest case, the reduction is analogous to the procedure whereby two ellipses, described by homogeneous co-ordinates, are referred to the same self-conjugate triangle of reference.

² When its determinant $|R|$ does not vanish, a matrix, R , is said to be non-singular. It then possesses an inverse or reciprocal, R^{-1} , such that $RR^{-1} = R^{-1}R = I$ (the unit matrix). To calculate R^{-1} , find first the cofactors r^{ij} (say) of the various elements r_{ij} in the matrix R ; transpose the entire matrix of cofactors: then the (ij) th element in R^{-1} is $\frac{r^{ji}}{|R|}$. This is the familiar formula giving the regression coefficient as the ratio of two determinants ((33), p. 296, equation (263)). For calculating the determinants the simplest methods are Aitken's ((35), p. 45) or Fisher's ((20), p. 150); but, as indicated later on, these calculations may be avoided.

of R , the latent roots of R^{-1} will be the reciprocals of the latent roots of R ((26), iii, p. 354). Hence $\lambda_1^{\frac{1}{2}}, \lambda_2^{\frac{1}{2}}, \dots, \lambda_f^{\frac{1}{2}}$ will be the principal semi-axes of the frequency ellipsoid, and so measure its standard deviations. Under these conditions, therefore, the factors deduced above are in effect the same as the factors deduced by the method of least squares and as the 'principal components' derived by Hotelling's method. For a direct demonstration, see the memorandum already cited ((13), pp. 289 f.).

But the underlying theory gains a wider application if, as I have already suggested, we regard the resulting geometrical figure, not as arising specifically from a normal distribution of the variables, but more generally as a strain or tensor ellipsoid describing the multi-dimensional distortion, produced by our methods of measurement—methods that have the effect of stretching the distribution of original factor measurements obliquely, and even (under certain conditions) of projecting them into a space of a different number of dimensions. Moreover, for purposes of calculation, as well as of demonstration, the procedure given above (using R instead of R^{-1}) is far simpler as well as more general. The chief difficulty—not, I think, a very serious one—is the indeterminacy that is introduced when $f < t$. It forms my chief reason for preferring the alternative and somewhat more circuitous proof given in a later section.

Having, then, determined V by solving equation (19), we have now two sets of equations to determine the factor loadings F , namely, $FF' = R$ and $F'F = V$. From F we may deduce the semi-orthogonal matrix of direction cosines L , by simply 'rectifying' F by equation (11) which we can rewrite $L = FV^{-\frac{1}{2}}$: we have only to divide each column of factor loadings by the root of its own unaveraged variance. Alternatively, we may determine L direct from R and V by the equations

$$(R - \lambda I) L = 0 \quad \text{.....(21)}$$

and then deduce F by equation (11).

If the number of factors is large, to calculate the determinants in equation (19) may be almost impracticable. In such a case we may reach a solution by taking product-moments of a higher order. From (17) and (12) we have

$$R^m = (FF')^m = FV^{m-1}F'. \quad \text{.....(22)}$$

Thus, if $v_1 > v_2 \dots > v_f$, on repeatedly multiplying the correlation-matrix by itself, we shall gradually reduce it to hierarchical form: how soon this is effected will depend on the excess of v_1 . We may then obtain the loadings or 'saturation coefficients' for the first factor by a simple summation formula (cf. (13), p. 284)

$$r_{kg} = \frac{t_k}{\sqrt{\sum t}} \times \frac{1}{v_1^{\frac{1}{2}(m-1)}}, \quad \text{.....(23)}$$

where t_k denotes the total of the k th column of R^m , or by the simpler formula

$$r_{kg} = \frac{\sqrt{d_k}}{(\sum d)^{(m-1)/2m}}, \quad \text{.....(23a)}$$

where d_k denotes the element in the leading diagonal under the k th test, and $\sum d$ the sum of all such elements. To obtain the loadings for the second factor, we take a smaller value for m , reconstruct the hypothetical table of product sums due to the

first factor only $(f_1 f_1')^m$, deduct this from the given table (R^m), and factorize the residuals by the same procedure. Note that, since $f_1' f_1 = v_1$, $(f_1 f_1')^m = v_1^{m-1} (f_1 f_1')$. The method is more fully explained and illustrated in (38).

By one or other of these methods, then, without any iterative procedure, we can calculate V , F and L . It remains to determine W .

(a) If $f=t$, both L and F will be square, non-singular matrices. Accordingly, since we have assumed $M = LP_1$, we have at once $P_1 = L'M$. (This is the algebraic generalization of Kelley's arithmetical equations: (33), p. 58). And again, if we prefer P in unitary standard measure, since we have assumed $M = FP$, we have $P = F^{-1}M$, that is

$$W = F^{-1}. \quad \text{.....(24)}$$

(b) We desire, however, to keep the number of factors as small as possible. But if $f < t$, $F = 0$; and we cannot use F^{-1} . We may nevertheless adopt the following method, though it is not beyond technical criticism. Multiplying both sides of equation (13) by L' , we have

$$L'M = L'LV^{\frac{1}{2}}P$$

$$= V^{\frac{1}{2}}P,$$

$$\text{that is,} \quad P = V^{-\frac{1}{2}}L'M, \quad \text{.....(25)}$$

or (substituting from equation (11))

$$= V^{-1}F'M. \quad \text{.....(26)}$$

Our initial equation, $P = WM$, will therefore be satisfied by taking

$$W = V^{-\frac{1}{2}}L' \quad \text{.....(27)}$$

$$= V^{-1}F'. \quad \text{.....(28)}$$

Thus we have at length reached a formula for estimating the measurements for the several persons in terms of the hypothetical factors assumed to underlie the correlations between the tests.

The weighting formula ($W = F'R^{-1}$) given in my previous memorandum ((13), p. 299, equation (xxxvii)) was devised to meet the case in which the number of factors was assumed to be greater than the number of tests, since there the scheme of factors ((13), p. 264) was constructed so as to contain specific factors (including errors) peculiar to each test, as well as the factors common to all or to groups. Writing $LV^{\frac{1}{2}}$ for F and $LV'L'$ for R as above, we have $W = V^{\frac{1}{2}}L'LV^{-1}L'$, which at once reduces to equation (27). If, with Spearman, we assume $(t+1)$ factors, i.e. one general factor and t specifics, the form given by equation (xxxvii) yields Spearman's original weighting formula ((31), p. xix, equation (29)) which was derived by the method of least squares: in that case Spearman's weighting equation for 'true g ' as distinct from estimated g becomes

$$g_x \equiv p_{1x} = \Sigma w_k m_{kx} + w_{t+1} m_{t+1},$$

where $w_{t+1}^2 = \frac{1}{\Sigma \{r_{kg}^2 / (1 - r_{kg}^2)\} + 1}$, and m_{t+1} is a set of indeterminate 'measurements', uncorrelated with the test measurements but otherwise quite arbitrary.

B. *Correlating persons*

It has commonly been assumed that the same mode of factor analysis can be applied to correlations between persons as to correlations between tests. Here, however, we may easily be led to false or meaningless conclusions, if we work in terms of correlation rather than in terms of variance and covariance. First, we again require a definition of 'factors'. I suggest that the best set of factors will be the components yielding the widest discrimination between the traits actually tested or observed: this means, as before, maximizing the ratio of 'variance between arrays' to 'variance within arrays'. Secondly, it has not been noted that the results of factorizing correlations are not, in general, the same as the results of factorizing the original covariances from which the correlations have been derived.¹ Now, if we re-standardize our matrix of persons in order to obtain correlations between persons, we shall (save in exceptional circumstances) be substituting a different matrix. Hence, it is essential that the analysis be applied direct to the matrix of covariances, not to the matrix of correlations: otherwise, we may unconsciously prevent ourselves from ever obtaining the same set of factors for persons as for tests.

If these two points be admitted, we may follow the same procedure as before. Assuming $M' = F_p P_p$ we have already obtained (equation (14)) $M' = L_p V_p^{\frac{1}{2}} P_p$, where L_p , $V_p^{\frac{1}{2}}$, P_p are to be deduced by factorizing R_p on the lines just explained for R_t .

When the factorization has been carried out, it will be found that $V_p = V_t$. This result can be most simply illustrated if we take for M an imaginary matrix of actual numbers constructed from known orthogonal or semi-orthogonal matrices L_t and P_t . Then

$$R_t = M M' = L_t V_t^{\frac{1}{2}} P_t \cdot P_t' V_t^{\frac{1}{2}} L_t' = L_t V_t L_t';$$

that is

$$V_t = L_t' R_t L_t, \text{ as already noted.}$$

$$\begin{aligned} \text{Similarly } R_p = M' M &= P_t' V_t^{\frac{1}{2}} L_t' \cdot L_t V_t^{\frac{1}{2}} P_t = P_t' V_t P_t \\ &= P_t' L_t' R_t L_t P_t. \end{aligned}$$

Now if L_t and P_t are orthogonal,² their product $L_t P_t$ will be orthogonal; and the characteristic determinant of R_p will be

$$| R_p - \lambda I | = | L_t P_t |^{-1} | R_t - \lambda I | | L_t P_t | = | R_t - \lambda I |.$$

¹ Even Kelley seems to assume that they are 'identical' ((33), p. 1): by comparing the characteristic matrices that arise, it is easy to show that the latent roots can only be identical when all the standard deviations are equal.

² This would imply that the number of persons tested shall be the same as the number of tests, and the number of tests the same as the number of presumable 'factors'. In that case, however, the measurements cannot be expressed as deviations about their own means,

Hence R_p and R_t will have the same characteristic equation and therefore the same latent roots, i.e. $V_p = V_t$. If $t \neq p$, determinoids must be substituted for determinants; but the same result is ultimately obtained. The general case, however, is more easily dealt with on the lines suggested below.

Assuming that the result can be generalized, we have, on correlating persons,

$$M' = L_p V_p^{\frac{1}{2}} P_p = L_p V_t^{\frac{1}{2}} P_p.$$

But, on correlating tests, we have (on transposing the matrices in equation (13), and remembering that $V_t^{\frac{1}{2}}$ is a diagonal matrix and therefore $= V_t'^{\frac{1}{2}}$)

$$M' = P_t' V_t^{\frac{1}{2}} L_t'.$$

And it is clear that a particular solution, satisfying these two equations, can at once be obtained by putting

$$P_p = L_t', \quad \text{.....(29)}$$

$$\text{and taking } L_p V_p^{\frac{1}{2}}, \text{ that is } F_p = P_t' V_t^{\frac{1}{2}}. \quad \text{.....(30)}$$

Thus, to determine factor loadings and saturation coefficients which will satisfy the correlations reached by correlating persons, we have no need to calculate this second set of correlations and undertake a fresh factor analysis, but may always assume that they can be deduced from the results reached by correlating tests.

(2) General solution

The foregoing proof has been given as being at once the simplest to grasp and the most in keeping with the procedure usually followed in factorizing correlations in psychology. It is, however, at certain points lacking in generality and rigour. Accordingly, I shall now attempt a proof which will be less open to technical criticism, but at the same time and of necessity somewhat more difficult for the general reader.

The central point to establish is the identity of the latent roots of the two covariance matrices. This may be proved directly, and independently of any theory as to the structure of M , as follows. Let us compare the coefficients in the expansions of the two characteristic equations of R_t and R_p respectively, as given by equation (19). Since the rank of both R_t and R_p is f , there will be f coefficients in each equation. Consider any though they might be expressed as deviations about the means of the total populations; for, if R contains the product-sums about means deduced from the same table, t, p, f cannot all be exactly equal: (*v. p. 74, fn. 3*). But, for an arithmetical illustration, the results obtained (it will be found) are close enough to make the argument intelligible and plausible to those who cannot follow the more technical proof given in the next section.

one of these coefficients, say $(S_k)_t$, the k th coefficient in R_t . It is, as we have seen, the sum of all the k -rowed diagonal minors in R_t . For simplicity, take the case in which $t=3$ and $k=2$, and consider any one of these minors. Since $R_t=MM'$, the 2-rowed minor

$$\begin{aligned} \begin{vmatrix} r_{11}, r_{12} \\ r_{21}, r_{22} \end{vmatrix} &= \begin{vmatrix} m_{11}^2 + m_{12}^2 + m_{13}^2, & m_{11}m_{21} + m_{12}m_{22} + m_{13}m_{23} \\ m_{21}m_{11} + m_{22}m_{12} + m_{23}m_{13}, & m_{21}^2 + m_{22}^2 + m_{23}^2 \end{vmatrix} \\ &= \begin{vmatrix} m_{11}, m_{12} \\ m_{21}, m_{22} \end{vmatrix}^2 + \begin{vmatrix} m_{11}, m_{13} \\ m_{21}, m_{23} \end{vmatrix}^2 + \begin{vmatrix} m_{12}, m_{13} \\ m_{22}, m_{23} \end{vmatrix}^2. \end{aligned}$$

Now the last expression consists of the sum of the squares of all the 2-rowed determinants that can be formed from that particular 2-rowed submatrix in M , which, when multiplied by its transpose, yields the 2-rowed product-minor in question. And the sum of all such 2-rowed product-minors will be the sum of all such expressions. It is clear that this equivalence can be generalized, no matter what be the number assigned for t , and no matter how many rows there are in the minor determinant. Hence,

$$(S_k)_t = \sum_{i,j} |m_{ij}|^2, \quad \dots\dots(31)$$

where i denotes any sequence of k numbers, taken from the series of row-numbers 1, 2, ..., t always in the same ascending order, and j denotes any sequence of k numbers similarly selected from the column-numbers 1, 2, ..., p .

Similarly in the matrix R_p

$$(S_k)_p = \sum_{j,i} |m_{ji}|^2. \quad \dots\dots(32)$$

But the expressions on the right of equations (31) and (32) are equal.

Hence $|R_t - \lambda I| = (-1)^t \{\lambda^t - (S_1)\lambda^{t-1} + \dots + (-1)^t (S_t)\}$,

and $|R_p - \lambda I| = (-1)^p \{\lambda^p - (S_1)\lambda^{p-1} + \dots + (-1)^p (S_p)\lambda^{p-t}\}$.

Thus $|R_p - \lambda I| = (-\lambda)^{p-t} |R_t - \lambda I|$.

Therefore, R_p will have at least $(p-t)$ zero latent roots, and the rest will be identical with the t latent roots of R_t ; if R_t has a rank of f , then $(t-f)$ of these will also be zero. Thus, even when t , p and f are unequal, $V_p = V_t$.

Now, in the previous proof, I have followed the usual practice, calculating first the saturation coefficients or factor loadings, F , and then deducing from these the weights or regression coefficients, W . But for theoretical purposes, to begin with the equation $M=FP$, when P is not

given and M is, seems somewhat illogical: it is more natural to start from the equation $P = WM$, and deduce the conditions which P must fulfil.

This keeps the procedure employed when factors are deduced from the test measurements themselves parallel with that which is adopted when an external criterion is available. There it is usually assumed that $|R| \neq 0$. If $|R| = 0$, then we should have to take into account both the rank of M and the dependent linear relations arising between the linear functions involved. But, by following the standard procedure for solving such equations ((26), i, pp. 366 f.), a general solution could still be reached, with the usual conditions attached. The degrees of arbitrariness that may then be involved will be realized if the reader considers the case in which the rank of M (and therefore of R) is $f=1$. Now, when an external criterion is not available and $|R| \neq 0$, much the same procedure is followed for finding the regression coefficients as before (cf. (13), p. 272). But once again, if $|R| = 0$, we must consider the rank of M and the dependent linear relations arising.

Our two initial assumptions are that the various factors are uncorrelated, and that their (unaveraged) variances are unity. Accordingly, we have for our starting point the equation

$$WMM'W' = PP' = I. \quad \text{.....(33)}$$

Thus P^p must be a semi-orthogonal matrix, and has the character of a completely orthogonal matrix ${}_pP^p$ with $(p-f)$ rows deleted.

If neither t nor $p \equiv f$, ${}_jW^t$ and ${}_iM^p$ will be dissimilar and degenerate matrices.¹ Following the normal procedure ((26), iii, pp. 318 f.), the first stage in the solution will be to express them in terms of two square matrices, of similar order and with non-vanishing determinants, from which they may be derived.

Since f is the rank of ${}_jW^t$, it will be possible to select f columns, and not more than f , that are linearly independent² ((26), iii, p. 276, theorem v). Without loss of generality, we may take these to be the first f columns, calling them W_1 , and the remainder W_2 respectively. Thus W may be partitioned into two submatrices and written

$$W = [W_1, W_2], \quad \text{.....(34)}$$

where W_1 and W_2 have the form ${}_jW_1^t$ and ${}_iW_2^{t-f}$.

To reduce M two steps will be requisite. First, it is always possible to find an undegenerate matrix, ${}_iF^j$, such that

$$FF' = MM'. \quad \text{.....(35)}$$

F when found may in turn be partitioned into a square submatrix, F_1 say, consisting of the first f linearly independent rows and a dependent

¹ A degenerate matrix is one the rank of which is less than the number of its horizontal rows or vertical columns whichever is the smaller. It has no inverse; hence the need for the reduction described in the text.

² See p. 76, footnote 1, above.

remainder which may be written CF_1 . Thus F may be expressed in the form

$$F = \begin{bmatrix} I \\ C \end{bmatrix} F_1. \quad \dots\dots(36)$$

Since F_1 is a square matrix of rank f , $|F_1| \neq 0$ and F_1 has an inverse.

We have, then, $WMM'W' = WFF'W' = I$,

or $WF = \Theta$,

where Θ may be any orthogonal matrix of order f ; and the general solution to such an equation is normally obtained ((26), i, pp. 327 f.) by post-multiplying both sides by the inverse of an undegenerate minor matrix contained in the post-factor of W , namely, by F_1^{-1} . The details of the proof may be set out as follows:

On partitioning W and F as above, the equation to be solved may be written

$$\begin{bmatrix} W_1 & W_2 \end{bmatrix} \begin{bmatrix} I \\ C \end{bmatrix} F_1 = \Theta,$$

that is $\begin{bmatrix} W_1 I + W_2 C \end{bmatrix} F_1 = \Theta$.

On post-multiplying both sides by the inverse of F_1 , we have

$$W_1 = \Theta F_1^{-1} - W_2 C.$$

This gives the first f columns of W in terms of Θ and F_1 , and the remaining $(t-f)$ columns as arbitrary quantities. To obtain the complete matrix W , we may reintroduce W_2 on both sides, getting

$$\begin{aligned} W &= [W_1, W_2] = [(\Theta F_1^{-1} - W_2 C), W_2] \\ &= \Theta F_1^{-1} - W_1 [C, -I]. \end{aligned}$$

On multiplying by M throughout, we have

$$P = WM = \Theta F_1^{-1} M - W_1 [C, -I] M. \quad \dots\dots(37)$$

Now, in this equation, a consideration of the linear relations necessarily obtaining between the rows of the oblong matrix F will show that the term containing the arbitrary quantities $= 0$. For, since F has t short rows and rank f ($f < t$), there must be $(t-f)$ independent linear relations between its short rows ((26), i, p. 276, theorem vi). If $cF = 0$ expresses one of these relations (c denoting the vector $[c_1, c_2, \dots, c_t]$), then

$$cMM'c' = cFF'c' = 0.$$

But the expression on the left is a sum of squares: for the product $cM = [c_1, c_2, \dots, c_t] M$ may be written as a single vector $[\gamma_1, \gamma_2, \dots, \gamma_t]$; so that $cMM'c' = \gamma_1^2 + \gamma_2^2 + \dots + \gamma_t^2 = 0$. Therefore, since the quantities are all real,

$$\gamma_1 = \gamma_2 = \dots = \gamma_t = 0,$$

i.e. the product vector $cM = 0$ identically. \dots\dots(38)

$$\text{Now} \quad [C, -I] F = [C, -I] \begin{bmatrix} I \\ C \end{bmatrix} F_1 = 0.$$

Hence, by the preceding paragraph,

$$[C, -I] M = 0.$$

Consequently, the last term in (37) may be dropped; and we may take

$$P = M = \Theta F_1^{-1} M, \quad \dots\dots(39)$$

whatever value W_1 may have: (a 'conventional' equality since the redundant rows of M are ignored).

The general solution for P is thus indefinite, but only to the extent that an arbitrary orthogonal matrix may be prefixed to any particular solution that satisfies the initial equation. This indefiniteness is merely an expression of the well-known fact that, unless special conditions are imposed, we may freely rotate the axes of reference. Consequently, when we have found any particular semi-orthogonal matrix of suitable vertical and horizontal orders that will satisfy our initial equation (33), we can forthwith write down the general solution by prefixing an orthogonal matrix whose rank and order are the same as the rank of M .

Note that, if $t=f$, the equation $WM = \Theta F_1^{-1} M$ becomes

$$WM = \Theta F^{-1} M$$

(where F now has an inverse). And, since the rank of M is then equal to its passivity, M may be legitimately cancelled ((26), i, p. 35), and we have $W = \Theta F^{-1}$. We may choose I for the arbitrary matrix Θ , and so obtain, as the simplest solution for this special case, $W = F^{-1}$, which is the particular solution deduced above (equation (24), p. 80).

Since by definition $FF' = MM' = R$, the latent roots of R must also be the latent roots of FF' . Thus, we may take

$$FF' = LVL',$$

and

$$F = LV^{\frac{1}{2}},$$

where (if $f \neq t$) ${}_tL'$ will be, as before, an orthogonal matrix with its last $(t-f)$ columns deleted. Hence, from (36), we now have

$$F_1 = {}_tL' \cdot V^{\frac{1}{2}},$$

where ${}_tL'$ is a square matrix consisting of ${}_tL'$ with its last $(t-f)$ rows deleted. Thus we may take

$$W = \Theta F_1^{-1} = \Theta V^{-\frac{1}{2}} {}_tL'. \quad \dots\dots(40)$$

It will be observed that this gives a particularly simple weighting equation for deducing P : see illustrative example below, p. 94.

Let us now apply these results to the analysis of the correlations between tests and persons respectively. Distinguishing the two cases by the suffixes t and p as before, we have

$$F_t = L_t V^{\frac{1}{2}} \quad \text{and} \quad F_p = L_p V^{\frac{1}{2}},$$

where V denotes the latent roots of both R_t and R_p . But, since

$$L_p' L_p = I \quad \text{and} \quad L_p' \text{ has the form } {}_j L'^t$$

and

$$L_t' L_t = I \quad \text{and} \quad L_t' \text{ has the form } {}_j L'^p,$$

L_p' and L_t' are possible solutions to the initial equations for P_t and P_p respectively; and, in accordance with the preceding proof, the general solutions may be written

$$P_t = W_t M = \Theta_1 L_p', \quad \text{.....(41)}$$

and

$$P_p = W_p M' = \Theta_2 L_t', \quad \text{.....(42)}$$

where Θ_1 and Θ_2 are any square orthogonal matrices of order f . If, however, we have already found L_t and V by correlating tests, we can take W_t as given by equation (40) and so deduce a particular solution for P_t by the equation

$$P_t = W_t M = V^{-\frac{1}{2}} ({}_j L'_t) M, \quad \text{.....(43)}$$

and then, without correlating persons,

$$\text{take} \quad P_p = L_t'. \quad \text{.....(44)}$$

It will be observed that the values we thus finally reach are consistent with those given for P_t and P_p by equations (25) and (29).¹

V. CONCLUSIONS

The foregoing analysis throws light upon the nature of the hypothetical matrices into which our observed measurements are resolved. To regard them as measuring ideal mental traits (such as intelligence and the like) is no more legitimate than to regard them as measuring ideal persons. A vector of test measurements will specify either. Broadly speaking, it may be said that, if in the observed matrix (M) the rows describe actual tests and the columns actual persons, then in the calculated matrices (F and P) the rows and columns parallel to these may be regarded as describing hypothetical tests and hypothetical persons respectively.

(i) P_t . Let us examine the hypothetical factor measurements for persons first of all. Suppose that the observed F_t takes its simplest possible form, namely, the unit matrix ${}_j I'$. Then $M = F_t P_t = I P_t = P_t$. We

¹ I am deeply indebted to Dr T. Smith for his kindness in reading the drafts of my algebraic proofs and suggesting many welcome improvements.

should thus have f rows of measurements for f tests, the measurements being simply a reproduction of P_t . By analogy, whatever the value of F_t , we may always regard the f rows of P_t as in effect measurements such as would be given by f ideal and independent tests.

(ii) F_t . (a) Ordinarily the matrix of factor loadings is assumed to describe the way in which each actual test is 'loaded', 'weighted', or 'saturated' with the several component factors. In this sense F is a matrix of weights—regression coefficients by means of which we might deduce a person's actual measurements if we knew his hypothetical constitution.

(b) But it also yields a second piece of information. As we have seen (p. 75), if the initial measurements are expressed in unitary standard measure, F_t gives the partial correlations between the test measurements, on the one hand, and the factor measurements, on the other.

(c) In the general proof (p. 84), however, the matrix F_t was introduced, not as a matrix of factor loadings, but as a simplified form of M . From this point of view it may be regarded as containing a set of test measurements for persons, on the supposition that the persons have been chosen so that the measurements of each person are linearly independent of the rest.

For the empirically obtained P_t let us substitute the simplest possible value that P_t can take, namely, the unit matrix, I , with positive signs. This implies f persons in the group, each characterized fully and solely by a single factor, shared by no other person. On post-multiplying the observed F_t by this simplified matrix, we obtain, for M , a reproduction of the columns of F_t . Thus the columns of F_t represent the measurements obtained by f ideal persons, each a pure type whose constitution or behaviour is determined by a single factor. If we had taken negative signs for I , we should have obtained a second set of f persons—pure types that are the exact antithesis of the former.

(iii) P_p . The matrices derived by correlating persons may be given analogous interpretations. Thus, as each row in P_t describes the results of an ideal test, so each row in P_p describes the performances of an ideal person: it bears an interpretation the same as that just given for F_t , except that now the variances are all reduced to unity.

(iv) F_p . (a) The most natural interpretation of the matrix F_p is to regard it as a set of factor loadings for persons. From this standpoint the observed measurements of a particular person A are treated as a composite of the test measurements of several ideal persons (the 'factors'), each weighted according to the loadings for A .

(b) If, however, the initial units have been suitably adjusted, F_p gives saturation coefficients for persons, i.e. the correlations between each person's actual marks and the hypothetical marks he would have if his behaviour were determined solely by this or that particular factor. But these hypothetical marks, we have seen, are indicated in the columns of F_t . Hence the saturation coefficients for persons give in effect the correlations between each person's observed measurements in the several tests and the saturation coefficients for those tests. Thus F_t describes the norms or types; F_p describes each person's approximation to the several norms or types. This can be directly proved as follows. Since the $\sigma\sqrt{n}$ for M is 1 and for F_t is $V^{\frac{1}{2}}$, the correlation between actual measurements and saturation coefficients for tests [$r_{p,t}$] (say)

$$= M'F_tV^{-\frac{1}{2}} = F_pP_pF_tV^{-\frac{1}{2}} = F_pL_t'L_tV^{\frac{1}{2}}V^{-\frac{1}{2}} = F_p. \dots\dots(45)$$

(c) Just as F_t may be regarded as describing the performances of an ideal set of persons, so F_p may be regarded as describing the results of an ideal set of tests. Thus F_p gives for each person hypothetical measurements such as would be furnished by perfect tests for each factor. It differs from P_t only by weighting each factor measurement according to the amount of variance contributed by that factor. If, therefore, we have obtained F_p we need not calculate the regression coefficients and regression equations as is usually done to obtain the best estimates for the factor measurements; we need only apply the equation $P_t = F_pV^{-\frac{1}{2}}$.

Thus, at every point, it appears unnecessary to undertake a factor analysis both for correlations between tests and for correlations between persons. If, as is usually the case, the persons are more numerous than the tests, and the means of the tests themselves have no importance, it will be sufficient to analyse the covariances between the tests. Having obtained saturation coefficients for the tests, we may then deduce the saturation coefficients for the persons either from the hypothetical factor measurements obtained by regression equations in the usual way, or by correlating each person's observed marks with the saturation coefficients for the tests.

VI. ILLUSTRATIVE EXAMPLE OF WORKING METHOD

To illustrate the foregoing arguments in the concrete, I shall take a simple imaginary example, and describe the steps in a way which (I hope) will enable even the student who cannot follow the algebra to understand both the general line of proof and the working procedure.

Let M , the matrix of marks, be as follows:

Persons	...	(a)	(b)	(c)	(d)
Tests					
(1)		-6	2	0	4
(2)		3	1	-1	-3
(3)		3	-3	1	-1

(1) *Normal procedure*

I shall apply first the method described on pp. 72-82.

A. *Correlating tests*

1. The product sums for the rows, $R_i = MM'$ (equation (15)), are obtained as follows:

Test (1) and (1): $(-6)^2 + 2^2 + 0^2 + 4^2 = 56$;

Tests (1) and (2): $(-6 \times 3) + (2 \times 1) + (0 \times -1) + (4 \times -3) = -28$; etc.

Tabulating the sums, we obtain

Tests	...	(1)	(2)	(3)	Total
(1)		56	-28	-28	112
(2)		-28	20	8	56
(3)		-28	8	20	56
Total (after reversing negative signs)		112	56	56	224

The figures in the leading diagonal are the (unaveraged) variances and the other figures the (unaveraged) covariances: from these the coefficients of correlation can be obtained, if required, by the usual formula

$$\text{Correlation} = \frac{\text{Covariance}}{\sqrt{(\text{Product of the two variances})}}.$$

Thus, $r_{12} = -\frac{28}{\sqrt{(56 \times 20)}} = -0.84$, and so on.

2. To calculate the approximate factor loadings by what I have termed the 'summation formula' ((13), p. 284, equation (xxv)), we have here first to imagine the signs of the marks for tests (2) and (3) reversed: this reverses the signs of the correlations of those tests with the remainder, i.e. with test (1). For the first factor the summation formula then gives

$$\begin{aligned} \text{Factor loading for test (1)} &= \frac{\text{Sum of covariances for test (1)}}{\sqrt{(\text{Sum of all covariances})}} \\ &= \frac{112}{\sqrt{224}} = 2\sqrt{14}. \end{aligned}$$

Similarly, the factor loadings for tests (2) and (3), after restoring the signs, are $-\sqrt{14}$, $-\sqrt{14}$.

3. The factor loadings given by what I have termed the 'method of least squares' ((13), p. 287, equation (xxix)) are here the same. To verify this, use equation (22) putting $m=1$: i.e. take the loadings just found as trial values, multiply each row of covariances by the corresponding loading, add the products, and multiply each total by the ratio of the sum of the trial loadings to the sum of all the products (reversing signs if necessary): thus

Test	Trial loading	Cov.	Prod.	Cov.	Prod.	Cov.	Prod.
(1)	$2\sqrt{14}$	56	$112\sqrt{14}$	-28	$-56\sqrt{14}$	-28	$-56\sqrt{14}$
(2)	$-\sqrt{14}$	-28	$28\sqrt{14}$	20	$-20\sqrt{14}$	8	$-8\sqrt{14}$
(3)	$-\sqrt{14}$	-28	$28\sqrt{14}$	8	$-8\sqrt{14}$	20	$-20\sqrt{14}$
Total	$(+) 4\sqrt{14}$		$168\sqrt{14}$		$-84\sqrt{14}$		$-84\sqrt{14}$

Loading for test (1) (second approximation)

$$= \text{Total product for test (1)} \times \frac{\text{Total of trial loadings}}{\text{Total of products for all tests}}$$

$$= 168\sqrt{14} \times \frac{4\sqrt{14}}{336\sqrt{14}} = 2\sqrt{14}.$$

When the first approximation, as given by the summation method, does not yield the same figure as itself, we may proceed by successive approximations, taking the new figures as fresh trial values, until a sufficiently close agreement is attained. But in such a case (which arises when R deviates widely from hierarchical order) it is generally quicker to use the procedure described in equations (23) or (23a): see (38), pp. 185, 188.

4. In the present instance, since the determinants to be calculated are of a low order, it is easier to calculate the loadings (F_i) and direction cosines (L_i) directly, by the recognized method for discovering the 'latent roots'¹ of matrices. The 'characteristic equation',¹ when expanded in terms of the sums of the principal minors (equation (19)), is

$$-\lambda^3 + (56 + 20 + 20)\lambda^2 - 3(336)\lambda + 0 = 0,$$

or

$$-\lambda^3 + 96\lambda^2 - 1008\lambda = 0.$$

(The 1-rowed principal minors are 56, 20, 20. The 2-rowed principal minors—or 'tetrad-differences'—all = 336: e.g.

$$\begin{vmatrix} 56 & -28 \\ -28 & 20 \end{vmatrix} = 56 \times 20 - 28 \times 28 = 336.)$$

Solving the characteristic equation, we obtain for the latent roots $\lambda = 84, 12, \text{ or } 0$.

5. The values for R and λ_1 are then substituted in equation (21), which is written out explicitly as follows. A 'characteristic lambda matrix'¹ is

¹ These convenient terms are defined above, p. 78.

first formed by subtracting the first of these roots ($\lambda_1=84$) from the elements in the diagonal of the matrix of covariances; it is then post-multiplied by the first column of direction cosines (l_1, m_1, n_1). This gives the three equations for determining these first direction cosines.

$$\begin{aligned}(56-84) l_1 - 28m_1 - 28n_1 &= 0, \\ -28l_1 + (20-84) m_1 + 8n_1 &= 0, \\ -28l_1 + 8m_1 + (20-84) n_1 &= 0, \\ l_1^2 + m_1^2 + n_1^2 &= 1.\end{aligned}$$

Taking the sign of l_1 to be positive, we have, on solving these equations,

$$\begin{aligned}l_1 &= \frac{2}{\sqrt{6}}, \\ m_1 &= -\frac{1}{\sqrt{6}}, \\ n_1 &= -\frac{1}{\sqrt{6}}.\end{aligned}$$

Forming three similar equations by subtracting $\lambda_2=12$, we have

$$\begin{aligned}l_2 &= 0, \\ m_2 &= \frac{1}{\sqrt{2}}, \\ n_2 &= -\frac{1}{\sqrt{2}},\end{aligned}$$

the signs of the latter being determined by the equation

$$l_1 l_2 + m_1 m_2 + n_1 n_2 = 0.$$

6. For the matrix of factor loadings, $F_t = L_t V^{\frac{1}{2}}$ (equation (11)), we multiply the columns just obtained by $\sqrt{84}$ and $\sqrt{12}$ respectively, thus getting:

Test	Factor (i)	Factor (ii)
(1)	$2\sqrt{14}$	0
(2)	$-\sqrt{14}$	$\sqrt{6}$
(3)	$-\sqrt{14}$	$-\sqrt{6}$

7. For the regression coefficients, $W = V^{-\frac{1}{2}} L_t'$ (equation (27)), we rewrite the columns in paragraph 5 as rows and multiply the first row by $\frac{1}{\sqrt{14}}$ and the second by $\frac{1}{\sqrt{6}}$, or, using $W = V^{-1} F_t'$ (equation (28)), rewrite the columns in paragraph 6 as rows and multiply by $\frac{1}{14}$ and $\frac{1}{6}$, obtaining:

Test ...	(1)	(2)	(3)
Factor (i)	$\frac{1}{3\sqrt{14}}$	$-\frac{1}{6\sqrt{14}}$	$-\frac{1}{6\sqrt{14}}$
Factor (ii)	0	$\frac{1}{2\sqrt{6}}$	$-\frac{1}{2\sqrt{6}}$

8. To determine the factor measurements $P_t = W_t M$ (equation (4)), we multiply the figures in the rows of paragraph 7 by the figures in the columns of the initial matrix M , and add:

$$\text{thus, } \frac{1}{3\sqrt{14}} \times (-6) - \frac{1}{6\sqrt{14}} \times 3 - \frac{1}{6\sqrt{14}} \times 3 = -\frac{3}{\sqrt{14}}.$$

This gives, as the final result:

Persons ...	(a)	(b)	(c)	(d)
Factor (i)	$-\frac{3}{\sqrt{14}}$	$\frac{1}{\sqrt{14}}$	0	$\frac{2}{\sqrt{14}}$
Factor (ii)	0	$\frac{2}{\sqrt{6}}$	$-\frac{1}{\sqrt{6}}$	$-\frac{1}{\sqrt{6}}$

B. Correlating persons

Following the same procedure as has just been used for correlations between tests, we obtain the following:

9. The product sums for the columns, $R_p = M' M$ (equation (16)), are

Persons	(a)	(b)	(c)	(d)
(a)	54	-18	0	-36
(b)	-18	14	-4	8
(c)	0	-4	2	2
(d)	-36	8	2	26

10. The characteristic equation for R_p (equation (19)) is

$$-\lambda^3 + (54 + 14 + 2 + 26)\lambda^2 - 1008\lambda + 0 = 0,$$

which yields the same roots as before, 0, 84 and 12.

Comparing the coefficients in this equation with those of the characteristic equation for R_t (see above, paragraphs 1 and 4) note that

$$54 + 14 + 2 + 26 = 56 + 20 + 20 = 84 + 12,$$

and

$$1008 = 3(12^2 + 6^2 + 6^2 + 2^2 + 10^2 + 4^2),$$

thus exemplifying equations (20), (31) and (32) (pp. 78 and 83).

11. The direction cosines, L_p , are obtained as in paragraph 5:

Persons	Factor (i)	Factor (ii)
(a)	$-\frac{3}{\sqrt{14}}$	0
(b)	$\frac{1}{\sqrt{14}}$	$\frac{2}{\sqrt{6}}$
(c)	0	$-\frac{1}{\sqrt{6}}$
(d)	$\frac{2}{\sqrt{14}}$	$-\frac{1}{\sqrt{6}}$

But these are the figures obtained in paragraph 8, when we deduced the factor measurements P_t from the correlation between tests.

12. The factor loadings, $F_p = L_p V^{\frac{1}{2}}$ (equation (11)), are

Persons	Factor (i)	Factor (ii)
(a)	$-3\sqrt{6}$	0
(b)	$\sqrt{6}$	$2\sqrt{2}$
(c)	0	$-\sqrt{2}$
(d)	$2\sqrt{6}$	$-\sqrt{2}$

13. From which we obtain W_p and thence P_p as before:

Test ...	(1)	(2)	(3)
Factor (i)	$\frac{2}{\sqrt{6}}$	$-\frac{1}{\sqrt{6}}$	$-\frac{1}{\sqrt{6}}$
Factor (ii)	0	$\frac{1}{\sqrt{2}}$	$-\frac{1}{\sqrt{2}}$

But these are the figures obtained in paragraph 5 for L'_i by correlating tests.

(2) Alternative procedure

I shall illustrate the alternative procedure suggested by the 'general solution' (pp. 84-7) by recalculating P_p as obtained by correlating persons.

14. Let us take the second and fourth rows in the matrix F_p to be independent, namely,

$$\begin{array}{cc} \dot{\sqrt{6}} & \dot{2\sqrt{2}} \\ 2\dot{\sqrt{6}} & -\dot{\sqrt{2}} \end{array}$$

Then, taking $W_p = (F_1^{-1})_p$ (equation (40)), we have for $(F_1^{-1})_p$

$$-\frac{1}{5\sqrt{12}} \begin{bmatrix} 0 & -\sqrt{2} & 0 & -2\sqrt{2} \\ 0 & -2\sqrt{6} & 0 & \sqrt{6} \end{bmatrix} = \begin{bmatrix} 0 & \frac{1}{5\sqrt{6}} & 0 & \frac{2}{5\sqrt{6}} \\ 0 & \frac{2}{5\sqrt{2}} & 0 & -\frac{1}{5\sqrt{2}} \end{bmatrix}.$$

15. Post-multiplying W_p by M' (equation (42)), we obtain P_p as follows:

$$\begin{array}{cc} \begin{array}{cc} 0 & \frac{1}{5\sqrt{6}} & 0 & \frac{2}{5\sqrt{6}} \\ 0 & \frac{2}{5\sqrt{2}} & 0 & -\frac{1}{5\sqrt{2}} \end{array} & \begin{array}{ccc} \begin{array}{ccc} -6 & 3 & -3 \\ 2 & 1 & -3 \\ 0 & 1 & 1 \\ 4 & -3 & -1 \end{array} \\ \hline \begin{array}{ccc} \frac{2}{\sqrt{6}} & -\frac{1}{\sqrt{6}} & -\frac{1}{\sqrt{6}} \\ 0 & \frac{1}{\sqrt{2}} & -\frac{1}{\sqrt{2}} \end{array} \end{array} \end{array}$$

This procedure yields the same figures for P_p as were obtained in paragraph 13, by taking $W_p = V^{-1}F_p'$.

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(Manuscript received 10 August 1936)

THE STATISTICAL CONCEPTION OF MENTAL FACTORS

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- I. *The degree of reality of statistical concepts* (pp. 97-98).
- II. *Symbolism and estimation of factors* (pp. 98-100).
- III. *The interpretation of factors in the sampling theory of ability* (pp. 100-103).
- IV. *Summary* (p. 104).
- References* (p. 104).

I. THE DEGREE OF REALITY OF STATISTICAL CONCEPTS

In a previous paper (1) dealing with the estimation of ability in Spearman's two-factor theory, it was naturally convenient to assume the truth of this theory; with its justification I was not there concerned. But while the amount of experimental justification that is ascribed to it is not a problem with which I am qualified to deal, the actual interpretation of this and other factor theories has statistical aspects on which I should like to be permitted to offer comment.

Whatever degree of objectivity is attributed to the two-factor theory, it is recognized that this theory is not sufficient, group factors being introduced as auxiliaries when this is found necessary. Since a description of tests in terms of a general and specific factors *and* group factors is necessarily always possible, it has been questioned whether we can associate any reality with our estimates of these hypothetical factors, or whether they are merely convenient mathematical descriptions of our observed test scores. Thus Thomson(2) has said: "The factor language is convenient and vivid. But I may, I hope, be allowed to warn against the danger of personifying these factors, which are, I think, only mathematical coefficients." While tending to agree with this warning, I would at the same time point out that there is no *a priori* objection to any coefficient which appeared permanently and satisfactorily measurable in practice being regarded as possessing a certain degree of reality. (The temperature of a body is thought of as something real, but theoretically it appears merely as a mathematical coefficient in an equation of statistical equilibrium.)

There is certainly no reason to believe that general and specific factors are rigid and unanalysable concepts. It was stressed in (1) that general ability must always be a contrast with the statistical average of the group of persons tested; and further, if any statistician adopts the reasonable point of view that when estimating G he must consider specific factors as part of his experimental error, I doubt whether he will regard them as other than syntheses of components which (he hopes) will aggregate into random and normal variates. This statistical interpretation of factors might, however, be a very real one. The very illuminating remarks of Thomson(3) on his sampling theory of abilities seem to me to offer one possible theoretical justification of the factor postulates.

II. SYMBOLISM AND ESTIMATION OF FACTORS

In progressive work aiming at the analysis of test performance and the detection of 'factors', various semi-empirical methods of analysis, such as, for example, Hotelling's analysis of variates into their principal components, have already proved of great value. It is reasonable, however, to regard as one ultimate goal of such work the postulation of a set of factors in terms of which performance can be adequately accounted for, although at the same time factors which have established a sufficiently strong position to be regarded as entities must clearly depend on the results of very extensive experimental research.¹

¹ In a recent paper Stephenson(6) has attempted to make a distinction between research in type and individualistic psychology, by associating with the latter the usual factor technique and with the former what he has termed "the inverted factor technique". Thus on the postulation of a single general factor, whereas the usual technique enables us to estimate the score for any particular person in an ideal test typical of the tests used, and hence of a larger set of which these tests are a sample (cf. my remarks in § III), the inverted technique would enable us to estimate the score in any particular test for an ideal person typical of the persons tested, and hence of a larger set of which these persons are a sample. It has of course to be shown that such an estimated 'typical person' is a better concept than the 'average person' who scores the average marks in each test (for only a single general factor, I am doubtful whether it would be). Further, to avoid the difficulty of standardizing a set of scores which refer to different tests, Stephenson cited as a *set* of tests the grading of similar articles, such as a series of coloured papers, which grading could almost be regarded as a single test. Ignoring these points, which do not detract from the interest and possible value of the method, I do not think I am sure at present what the distinction drawn by Stephenson is intended to imply.

The chief value of the idea of psychological types must lie, not in an arbitrary classification of men into two or more grades, but in a demonstration that independent classifications of this kind (by, for example, the estimation of factors) are correlated. If from physical tests it was found possible to classify men according to a factor corresponding to Kretschmer's contrast of *pyknic* and *asthenic* types (the main general factor emerging from such tests

The symbolism of vector and matrix theory, which has been used extensively by Thomson, is very suited to expositions of factor theories. Thus we write

$$T = MF, \quad \dots\dots(1)$$

where T is a column vector denoting a series of standardized tests, F the postulated column vector of standardized factors, and M the matrix of correlations (supposed known) between tests and factors. Any estimate of F obtained from linear combinations of the tests will be of the form

$$f = AT.$$

From equation (1), and the formula

$$MM' = R,$$

where R is the matrix of test correlations, Thomson (4) has derived the identity

$$M(M'R^{-1}T - F) = 0,$$

of which a consistent solution is

$$f = M'R^{-1}T. \quad \dots\dots(2)$$

This he has put forward as an equation to estimate F .

If, however, we adopt the principle used in (1), that specific factors should be introduced only in order to explain discrepancies between observed scores and postulated general or group factors, we write

$$T = M_0 F_0 + M_1 F_1, \quad \dots\dots(3)$$

where F_0 is the set of general and group factors, and F_1 are the specifics. M_1 is thus a diagonal matrix. For two group factors G and H , the ordinary algebraic relation for the p th test is given by

$$T_p = (\rho_{pG}G + \rho_{pH}H) + \kappa_p S_p,$$

where

$$\kappa_p = \sqrt{(1 - \rho_{pG}^2 - \rho_{pH}^2)}.$$

Minimizing the sum of squares ΣS_p^2 , we obtain the equations

$$\Sigma \rho_{pG} (T_p - \rho_{pG}G - \rho_{pH}h) / \kappa_p^2 = 0,$$

$$\Sigma \rho_{pH} (T_p - \rho_{pG}G - \rho_{pH}h) / \kappa_p^2 = 0,$$

where g and h are our estimates.

would presumably be one merely of pure size), and it was also found possible to make an independent classification of temperament by means of mental tests, a significant correlation between the physical and mental factors would have provided a statistical justification in ordinary persons of Kretschmer's theory of types. Such a procedure would, it will be noticed, have conformed to orthodox test and factor techniques; it might in fact be regarded as a particular case of the general factor method of showing that two tests, or two series of tests, are not independent, but are better explained in terms of an underlying 'factor', though the case is a very special one because of the natural dichotomy of physical and mental measurements.

In matrix notation for any number of factors,

$$f_0 = (M_0' M_1^{-2} M_0)^{-1} M_0' M_1^{-2} T. \quad \dots\dots(4)$$

This equation represents our estimates for *all* our persons, if more generally we regard T as the matrix set of all the persons' test scores.

It was pointed out in (1) that the principle of estimation adopted there did not completely agree with the solution that has usually been employed, although the difference did not affect the relative weights assigned to the tests in estimating G . When group factors are introduced, however, the discrepancy between equations (2) and (4) is more serious. One point of view appears to have been to consider all the persons with different possible factorial make-ups that would give rise to the observed test scores of a particular person, whereas I have regarded the test scores as a sample of all the possible scores that might have arisen for that person according to the different values of specific factors he may happen to have. If we are postulating a set of factors F_0 and are considering how to estimate this set, given a particular person's scores, this latter point of view seems to me the logical one. We know that the estimate in equation (4) of any group factor has the minimum standard error. If

$$J = M_0' M_1^{-2} M_0,$$

the information (reciprocal of the error variance) on, say, G is given by

$$I_G = |J| / J_{GG},$$

where J_{GG} is the cofactor of the element j_{GG} in $|J|$.

III. THE INTERPRETATION OF FACTORS IN THE SAMPLING THEORY OF ABILITY

From the idea that the mind is a synthesis of a large number of components of which any test may approximately be regarded as a random sample, Thomson (3) has shown that the tendency to zero tetrad differences that is often observed in practice must necessarily follow theoretically. In a simple form, if for convenience we assign equal weights to all of many statistically independent components, and four tests sample at random fractions p_1 , p_2 , p_3 and p_4 of these components, then the value of the correlation between tests 1 and 2 will obviously be

$$\rho_{12} = \sqrt{(p_1 p_2)} \quad \dots\dots(5)$$

if the number of components is sufficiently large, and

$$\rho_{12} \rho_{34} = \rho_{13} \rho_{24} = \rho_{14} \rho_{23}.$$

We note further that on the usual Spearman two-factor theory

$$\rho_{1G} = \left(\frac{\rho_{12}\rho_{13}}{\rho_{23}} \right)^{\frac{1}{2}} = \sqrt{p_1}, \quad \dots\dots(6)$$

which is the correlation of test 1 with an ideal test which samples all the components.

If, therefore, we are prepared to recognize a statistical interpretation of factors, and in particular of general ability G , and we assume the truth of the sampling theory, the natural *a priori* definition of G on this theory as the average for any person of all his components is the definition to which an application of the two-factor method inevitably leads. Equation (6), for example, indicates that we should proceed to estimate this average from tests by the standard formula.

This point may be stressed by verifying directly how we should attempt to estimate G , which we have defined by

$$G = \lambda \bar{\epsilon}, \quad \dots\dots(7)$$

where $\bar{\epsilon}$ is the mean of all components ϵ , and λ is a standardizing factor. Let us write as our estimate

$$g = a_1 T_1 + a_2 T_2 + a_3 T_3,$$

where for simplicity we consider three tests, and the test scores T are standardized. Then the average value of $(g - G)^2$ is proportional to

$$\begin{aligned} & (b_1 - 1)^2 p_1 (1 - p_2) (1 - p_3) + (b_2 - 1)^2 p_2 (1 - p_3) (1 - p_1) \\ & + (b_3 - 1)^2 p_3 (1 - p_1) (1 - p_2) \\ & + (b_1 + b_2 - 1)^2 p_1 p_2 (1 - p_3) + (b_2 + b_3 - 1)^2 p_2 p_3 (1 - p_1) \\ & + (b_3 + b_1 - 1)^2 p_3 p_1 (1 - p_2) \\ & + (b_1 + b_2 + b_3 - 1)^2 p_1 p_2 p_3 + (1 - p_1) (1 - p_2) (1 - p_3), \end{aligned}$$

where $b_1 \propto a_1 / \sqrt{p_1}$, etc. The b 's are introduced to counteract the effect of standardizing tests comprising different numbers of components. The term $p_1 (1 - p_2) (1 - p_3)$ is the fraction of components in test 1 not entering into tests 2 and 3, and so on (cf. (3)). Minimizing this variance, we obtain the equations

$$\begin{aligned} b_1 + b_2 p_2 + b_3 p_3 &= 1, \\ b_1 p_1 + b_2 + b_3 p_3 &= 1, \\ b_1 p_1 + b_2 p_2 + b_3 &= 1, \end{aligned}$$

of which the solution is $b_1 \propto 1/(1 - p_1)$, etc., or

$$a_1 \propto \frac{\sqrt{p_1}}{1 - p_1} = \frac{\rho_{1G}}{1 - \rho_{1G}^2}.$$

Specific abilities on this theory are simply the differences between the components sampled by particular tests, and the average G of all components. That is, we define for two tests

$$\begin{aligned} S_1 &= \lambda_1 (\bar{\epsilon}_1 - \bar{\eta}_1), \\ S_2 &= \lambda_2 (\bar{\epsilon}_2 - \bar{\eta}_2), \end{aligned} \quad \dots\dots(8)$$

where $\bar{\epsilon}_1$ is the average of components entering into test 1, and $\bar{\eta}_1$ the average of the remaining components. It is obvious by definition that S_1 and S_2 are each statistically independent of G . The correlation between S_1 and S_2 depends on the average value of the product

$$(\bar{\epsilon}_1 - \bar{\eta}_1) (\bar{\epsilon}_2 - \bar{\eta}_2).$$

If the number of components common to tests 1 and 2 is k , this value becomes

$$\frac{k}{p_1 p_2} - \frac{p_1 - k}{(1 - p_2) p_1} - \frac{p_2 - k}{(1 - p_1) p_2} + \frac{1 + k - p_1 - p_2}{(1 - p_1) (1 - p_2)},$$

and $\propto k - p_1 p_2$.

Thus if the two tests are each random samples from the entire set of components, so that $k = p_1 p_2$, the factors S_1 and S_2 are independent.

The obvious fact that any specific factor must necessarily be a contrast between performance in any particular test and a person's general ability is thus explicitly brought out by this sampling interpretation. It is not fair, for example, to cite the hypothetical case of a person who has specifics above the average in 1000 tests in order to indicate that for such a person our estimate of G would be too high (5), for such a case is a 'statistical impossibility'. The odds against any person being of such a type are $2^{1000} : 1$. The common-sense interpretation would be that if a person has high scores in 1000 tests, he has a high general ability rather than high specifics. In practice of course, where few tests are available, we do have to recognize that there will always be some persons for whom our estimates of G will be badly out.

The idea of a specific being merely a contrast may be compared with the point already stressed that a person's general ability is only a statistical contrast with the group of persons with whom he is associated.

It has been assumed above that the tests are samples from all the components, but of course if they were all samples from a particular region only, the tendency for zero tetrad differences would remain, and any postulated G could refer only to the properties of that region. The sampling interpretation has in fact been succinctly expressed by Thomson as follows: "From my point of view these tests are then drawing samples from the same sub-pool of our mind's abilities. If we stray from this pool,

and fish in other waters, we shall break the hierarchy; but if we sampled the *whole* pool of a mind, we should again find the tendency to hierarchical order."

If two tests are not completely random, but are sampling from the same fraction p of all the components, they will be correlated to a higher extent than on the two-factor theory, the correlation between them being

$$\rho_{12} = \frac{\sqrt{(p_1 p_2)}}{p}.$$

Suppose five tests have the true correlations

$$R = \begin{pmatrix} 1 & 0.80 & 0.60 & 0.40 & 0.30 \\ 0.80 & 1 & 0.50 & 0.33 & 0.25 \\ 0.60 & 0.50 & 1 & 0.40 & 0.30 \\ 0.40 & 0.33 & 0.40 & 1 & 0.20 \\ 0.30 & 0.25 & 0.30 & 0.20 & 1 \end{pmatrix}.$$

These give zero tetrads except for ρ_{12} being too high. Since on the random sampling idea

$$\rho_{12} = \sqrt{(p_1 p_2)} = \rho_{1G} \rho_{2G} = 0.50,$$

we have

$$p = 0.50/0.80 = 0.63.$$

Let us define a group factor

$$H = \mu (\bar{\epsilon}_H - \bar{\eta}_H),$$

where $\bar{\epsilon}_H$ is the average of components of the set sampled by tests 1 and 2, and $\bar{\eta}_H$ the average of the remainder. Then by similar arguments as before, H is a factor independent of G ; and the modified specific abilities defined as

$$S_1 = \mu_1 (\bar{\epsilon}_1 - \bar{\eta}_{1H}),$$

$$S_2 = \mu_2 (\bar{\epsilon}_2 - \bar{\eta}_{2H}),$$

where $\bar{\eta}_{1H}$ is the average of components of the set corresponding to H not entering into test 1, etc., are independent of G , H and each other. H thus denotes a contrast between the components sampled by the tests containing this factor, and the rest of the mind, or the part of the mind, sampled by all the tests.

For the set of hypothetical correlations above, since from the definition of H ,

$$\rho_{1H} = \sqrt{\left\{ \frac{p_1 (1-p)}{p} \right\}},$$

$$\rho_{2H} = \sqrt{\left\{ \frac{p_2 (1-p)}{p} \right\}},$$

we have

$$\rho_{1H} = 0.60, \quad \rho_{2H} = 0.50,$$

from which, as a check, we verify that the actual value of the correlation

$$\rho_{12} = \rho_{1G} \rho_{2G} + \rho_{1H} \rho_{2H} = 0.80.$$

IV. SUMMARY

A statistical conception of mental factors does not necessarily invalidate the search for factors to which a certain degree of reality may usefully be attributed. A method of estimation is given of a set of postulated factors corresponding to the matrix equation

$$T = MF.$$

A definition and interpretation of general, specific and group factors in terms of the sampling theory of ability is also given.

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(*Manuscript received 17 June 1936*)

PUBLICATIONS RECENTLY RECEIVED

A History of Psychology in Autobiography, Vol. III. Edited by CARL MURCHISON. Worcester, Massachusetts: Clark University Press; London: Humphrey Milford. 1937. Pp. xvii + 327. 22s. 6d. net.

The editor announces that this, the third volume, is to be the last of the series. Many students will greet the announcement with regret, for undoubtedly these volumes of more or less informal autobiographies have proved of great interest and assistance to the reader who is familiar with their subjects' writings but removed from their sphere of action. The present book contains contributions from James Rowland Angell, Frederic Charles Bartlett, Madison Bentley, Harvey A. Carr, Sancte De Sanctis, Joseph Fröbes, O. Klemm, Karl Marbe, Charles Samuel Myers, E. W. Scripture, Edward Lee Thorndike, John Broadus Watson and Wilhelm Wirth. As in the earlier volumes, the nature of the contributions varies greatly from writer to writer, some authors giving a large amount of detail about their lives and others confining themselves mainly to the story of their intellectual development and their general views about psychological problems. Most readers will certainly find the contributions of the first class the more interesting. The English reader will probably find the attractive account by C. S. Myers of his multifarious activities the most absorbing of the contributions to the present volume. From it he will learn that there is hardly a phase of present-day psychology in this country which Myers has not touched and influenced. Yet the modesty of Myers's article hardly does justice to the enormous and beneficent influence which he has consistently exerted.

The present volume stands well with the others, and all three should be included in every psychological library to which students have access in the early stages of their training. Photographs of the contributors—but not always very good ones—are included.

Handbook of Vocational Guidance. By C. A. OAKLEY and ANGUS MACRAE. With a section by EDITH O. MERCER, and a Preface by C. S. MYERS. London: University of London Press, Ltd. 1937. Pp. xvii + 337. 10s. 6d. net.

The authors of books on vocational guidance are necessarily faced with certain difficulties in their treatment of the subject. Should it be comprehensive, including all the relevant information, theoretical and practical, on the measurement and classification of individual differences? Or should it be selective and critical—either from a detached psychological point of view, or from a severely practical point of view? The standpoint adopted by the authors of this book is in the main the last of these three. But they have rather confused matters by including description of and references to theoretical and experimental material which can only be very indirectly useful in practical vocational guidance; while omitting in some cases a clear analysis and classification of certain abilities, which would have been of the greatest utility. Thus they mention that Alexander's factor of practical intelligence may be the same as Stephenson's *s* factor; and the latter may be either the ability to perceive form relations, or constructional ability. Are we to deduce that for practical purposes these are the same? And if so, how are they related to 'constructive ability' (see *I.H.R.B. Rep.* No. 33)? A similar confusion seems to exist with regard to the concepts of general intelligence as measured by verbal tests, the verbal factor and verbal

reasoning ability. Would it not be possible to attempt the analysis and classification of these abilities on a basis, not of mathematical criteria, but either of underlying psychological differences, or of immediately practical ones such as must present themselves constantly to the vocational guidance expert? A similar analysis is required of the abilities labelled 'aesthetic'; creative ability, technique, and power of appreciation of the arts almost certainly demand different forms of ability. Moreover, it has been clearly shown (see P. E. Vernon in *The Testing of Intelligence*) that they bear little relationship to the performance of simple sensory and perceptual tests such as Seashore's.

Those interested in practical vocational guidance would like more detailed information about the technique used in the interviews—for instance, about exact forms of questions to be used or avoided, since Muscio showed many years ago how important a factor this may be. But they will welcome the analysis and classification of the qualities and abilities required for various occupations, hoping that this occupational analysis may in future be greatly extended. Numerous excellent schedules of occupations with their openings and prospects have previously been published; but the actual psychological qualities which make for happiness and success in these occupations are usually a matter of guess-work rather than of investigation.

It must, then, be emphasized that this book, in spite of the criticism levelled at it, fulfils a most useful purpose, and will be very valuable both to those actually engaged in vocational guidance and to students seeking knowledge of the subject. It is to be hoped that the authors will be able to continue refining the technique and methods which they have so interestingly described.

M. D. V.

A Borstal Experiment in Vocational Guidance. By ALEC RODGER.
Medical Research Council, Industrial Health Research Board,
Report No. 78. London: H.M. Stationery Office. 1937. 9d. net.

This valuable report gives the results of an experiment executed in 1930. Four hundred boys were examined before they started their Borstal training. Group and individual tests were supplemented by an interview, and qualitative observations concerning temperamental characteristics were also recorded. One-half of the boys, chosen at random, served as a control group, and they were allowed to enter work parties in the ordinary way. The rest were definitely recommended as to which work parties they would be best suited for, and these recommendations were forwarded to the house master concerned.

Mr Rodger encountered many difficulties in the course of his investigation. He had limited knowledge of the requirements of Borstal work parties: there were no existing Borstal norms available for the various tests when the investigation was begun; the number of different kinds of parties was insufficient to suit all types of person—there being, for instance, no sales parties in which intelligent, persuasive and sociable individuals could be placed; vacancies were not always available in the work parties given first choice; the turn-over in house masters sometimes led to reports being given on boys with whom the house masters had only a very short acquaintance, and so on. Furthermore, Mr Rodger points out that in experiments of this kind the control groups are not perfect controls. In the course of an examination a boy might realize that his aptitude did not lie in the direction along which he thought it lay. Consequently he might become less inclined to ask for that kind of job than he would have been had no examination been made. This is a point it is well to bear in mind in connexion with other controlled experiments in vocational guidance.

These difficulties have the effect of diminishing the differences between the control and the recommended groups by an unknown amount. In spite of them, however, it was found that 69.5 per cent of the boys in the recommended group compared with only 45.6 per cent of those in the control group became Grade A workers. J. M. B.

Thanet Mental Tests. Prepared by W. P. ALEXANDER. London: University of London Press, Ltd. 1937. Handbook, 1s. net; School Aptitude Test, 4d.; Arithmetic Test, 6d.; English Test, 3d.

The *Thanet Mental Tests* have been prepared for the use of Education Authorities in selecting children for secondary education. They have been used by the author for children aged eleven years at Walthamstow, Margate, and in certain other areas. They comprise an aptitude or intelligence test, and minimum essentials tests in arithmetic and English. The aptitude test is designed to measure *g* in combination with *v* (verbal factor), since "there is evidence that these two factors are about equally important, relatively, in examination success in secondary schools". The arithmetic test gives simple computations, but no problem solving; the English test, tests for comprehension, punctuation and spelling, but there is no test for composition or grammatical expression. Instructions for administration and scoring and tables of norms are given. It is recommended that a pupil selected for secondary school education should have an I.Q., on the aptitude test, not below 115 and preferably above 120, and a score on the arithmetic and English tests not below normal.

Thus the Education Authority may by using these tests select for secondary education those who possess the ability to succeed in examinations; whether they can select those who possess intelligence, originality, imagination, or even the power to express themselves in their own language, appears more doubtful.

Handbook of Successful Preparation for Examinations. By S. S. JALOTA. Lahore: Careers. 1937. Pp. ii+92. Re. 1.

It is a little difficult to decide whether this kind of book can be of much practical use to candidates preparing for examinations. The good candidate will not need it, and the poor candidate, while he may be very much impressed by the excellent advice that is given, will not find it particularly easy to put the advice into practice. However, on the whole the discussion, though mainly on a very general level, is sensible and displays an adequate knowledge of much psychological experimentation. References for further study are included.

Fatigue and Boredom in Repetitive Work. By S. WYATT and J. N. LANGDON (assisted by F. G. L. STOCK). Medical Research Council, Industrial Health Research Board, Report No. 77. London: H.M. Stationery Office. 1937. Pp. iii+86. 1s. 3d. net.

In this further study of the nature and incidence of boredom among 355 women workers performing repetitive work, the authors extend their previous conclusions on the subject, and show that the amount of boredom experienced is associated with the following personal characteristics: intelligence, inability to mechanize simple manual processes, temperamental tendencies which are extroverted rather than introverted, a desire for creative as distinct from repetitive work. But even slight differences between one type of work and another may be responsible for widely differing amounts of boredom in the same worker. Boredom is relieved by day-dreaming, talking, singing, listening to gramophone music (which also produced an increase in output, greatest when the music was played for 75 min. in the middle of the spell).

The workers' answers to questionnaires relating to factors associated with the conditions of work and to causes of discontent showed that security of employment was the most valued condition, and waiting for work and troublesome material the most frequent causes of discontent. But the most bored workers attached relatively more importance to opportunities for promotion, opportunities to use their own ideas, and work which makes them think, and less importance to work which needs no

thought; and also reported many more causes of discontent. Thus it appeared that a vicious circle was set up between undesirable working conditions and unsuitable temperamental qualities in the individual, each reacting on the other.

This excellent piece of work throws further light upon susceptibility to boredom; it would be most interesting to know more of the exact nature of the personal characteristics producing susceptibility, and why the incidence is so different between one repetitive task and another.

Human Ecology. By J. W. BEWS. Oxford University Press; London: Humphrey Milford. 1936. Pp. xii + 312. 15s. net.

Whether one agrees or disagrees with many of the general contentions of this book, it is undeniably a contribution to social psychology and anthropology of intense interest and of considerable importance. Ecology is defined as dealing with "all the interrelationships of living organisms and their environment". Dr Bews is well known as a botanist of wide repute, and it is from the study of plant ecology that he takes his departure. Having defined 'environment' as comprehensively as possible and dealt with problems of heredity and response, he proceeds to a most comprehensive study of human ecology. He summarizes very well the growth and present position of psychology and describes the most important current movements and views in social anthropology. He then considers in turn: Ancient Man and his Ecology; the Ecology of the Food-Gatherers; the Ecology of Plant Cultivators; the Ecology of the Herdsmen and Pastoral Nomads; Ecological Differentiation and Classification, that is, some of the main classes into which environmental interplay has thrown men and women; and he finally sums up his main conclusions indicating what he takes to be the most important lines of development for future research. He makes it clear throughout that he considers that there are very important differences between human and plant ecology, and that these are at bottom due to the capacity for integration which belongs pre-eminently to the human organism. The same point is emphasized strongly by the Rt Hon. J. C. Smuts who contributes a very attractive foreword, claiming, with justice, that the whole book may be regarded as a further justification of a holistic philosophy of life.

The Psychology of a Suppressed People. By the Rev. J. C. HEINRICH. London: George Allen and Unwin, Ltd. 1937. Pp. viii + 148. 3s. 6d. net.

Everybody now agrees that what social psychology needs more than anything else is a lessened eagerness to relapse easily into sweeping generalization, and a greater desire to accumulate facts. Every serious social psychologist, therefore, will welcome this little book, with its direct study of the reactions of two large groups of oppressed people. Its general topic is a study of the behaviour of the Indian 'untouchables' and of the negro in the southern states of America. The argument is based partly upon first-hand observation and partly upon documents, full references being given. It is possible that some of the observations and references are biased, but that they represent a part of the truth seems certain enough. The author is endeavouring to study the behaviour of communities who are continually subjected to stimuli to which they cannot acquire actual unresponsiveness, but to which direct reaction involves real or imagined disadvantages. He illustrates the ways in which such a situation is bound to lead to resentment and fear, to concealment, and to various forms of indirect reaction, such as the effort to make a member of the dominant group or race look a fool. He studies the general characteristics of social inferiority reactions in concrete settings, and finally he attempts a more constructive sketch of how the difficulties might be combated by missionary enterprise in India. The book should certainly be read, and then, perhaps, supplemented by some study of the other side of the picture, if as good a representation of it can be found.

The Psychology of Feeling and Emotion. By CHRISTIAN A. RUCKMICK.
London: McGraw-Hill Publishing Co., Ltd. 1936. Pp. xiv + 529. 25s.

Emotional experience is of the utmost importance to the man-in-the-street and to the psycho-pathologist, yet its study has, comparatively speaking, been neglected by the professional psychologist in recent years, and this aspect of mental life has occupied relatively little space in text-books and time in teaching courses. The reason is, perhaps, the intrinsic difficulty of experimenting on the feelings and emotions, so that, though a considerable amount of experimental work has been attempted, the positive conclusions to be drawn are few. In consequence the teacher who is not specially interested in the affective life, is perplexed because there is no book to which he can refer for an account of the work which has been done.

This last difficulty at least has now been removed. Prof. Ruckmick has given us a clear, well-balanced and critical account, with its historical background, of the present state of knowledge and of theory regarding feeling and emotion.

After his introductory chapters and an excellent account of the work on feeling, Prof. Ruckmick proceeds to consider the emotions, beginning with the James-Lange-Sergi theory and the various resulting controversies, which naturally leads up to an account of the work of Cannon and Bard and their co-workers. He then outlines his own phylogenetic approach, with which the reviewer is in close sympathy, though it is a view which will meet with much opposition from those who are biased towards a behaviouristic conception. If one adopts this view one can understand why so much of the experimental work, which assumes, or seeks to find, a correlation between emotional experience and physiological change, is inconclusive and contradictory, and why even the facial expression of emotion is not to be relied on. It would seem, indeed, that the latter is but a conventional and socialized form of expression, like speech. Most of the work on these various problems is outlined and discussed critically and with understanding.

There are chapters also on the pathology of the affective life, the role of feelings and emotions in psycho-analysis, feelings and emotions in the animal mind and in the child, etc. But these are of lesser importance.

The Psychology of Feeling and Emotion is an excellent book which supplies a real need. Prof. Ruckmick is to be congratulated on the way in which he has completed an exceedingly difficult task.

H. B.

Surprise and the Psycho-analyst. By THEODOR REIK. London: Kegan Paul, Trench, Trubner and Co., Ltd. 1936. Pp. vii + 294. 12s. 6d. net.

This exposition of the underlying psychological bases of psycho-analytical method should be of the greatest value to the psycho-therapist, and is undoubtedly of great interest to other psychologists. The latter are often very sceptical of the nature of the diagnoses made by methods and from deductions that appear at first sight to be excessively arbitrary. But Dr Reik's detailed description of these methods makes these diagnoses appear less arbitrary, if still not altogether easy to credit. He describes how the therapist, maintaining an attitude of poised attention, is able through unconscious observation to conjecture the nature of the patient's unconscious processes. This conjecture appears as a sudden flash of comprehension of the latent meaning carried in the patient's words, and of their unconscious origin, and is accompanied by a feeling of surprise or shock in the analyst at its suddenness and strangeness. Dr Reik's discussion throws little light upon what actually is perceived in the process of unconscious observation, and it seems better at the moment to accept the existence of this process without trying to establish its exact nature. The psychological basis of the conjecture seems to be as follows: In response to the unconscious perception of the patient's impulses and emotions, there is a momentary transformation of the ego of the analyst, an introjection, followed by a projection of the transformed ego and its perception as a psychological object; this in turn produces the conjecture. It is necessary that the analyst should maintain throughout a state of detached

observation, of himself as well as of the patient, and of freedom from exact pre-conceived ideas as to the contents of the patient's unconscious mind. Sometimes there can also occur a "reciprocal illumination of conscious happenings", through which the analyst is able to understand consciously his own unconscious processes, as well as those of the patient, through an analysis of his spontaneous reactions to what the patient says.

Dr Reik discusses the methods of psychological interpretation of certain other analysts, and the criticisms which have been levelled at his methods. It is impossible to make any final judgement upon the heuristic value of these methods; still less upon the correctness of his theories as to the nature of the underlying processes. But there is no doubt as to the importance of understanding the nature and functioning of these processes, or of their very great interest to the psychologist.

An Introduction to Criminology. By W. A. BONGER. London: Methuen and Co., Ltd. 1936. Pp. xii+178. 6s. net.

This is an excellent translation of Prof. Bonger's book, which was published about four years ago in the Dutch University Extension Library.

After a general introduction, and an interesting chapter on the pre-history of criminology up to about the eighteen-thirties, come the more important chapters dealing with the various schools of criminology—the Italian school, as exemplified by Lombroso whose anthropological theory meets with devastating criticism; the French school which stresses environmental factors; the Biosociological school which, beginning with Ferri, a follower of Lombroso, considers crime to be the outcome of both individual and environmental factors; the Spiritualistic school which maintains that lack of religion is one of the causes of crime, a tenet which is shown to be untrue. The next chapter, on Criminal Psychology, is interesting and shows the need there is for further investigation.

Prof. Bonger is to be congratulated on the writing of a well-documented, readable and scholarly introduction to criminology.

There is a slight omission (of a line, or perhaps of two) at the bottom of p. 128. The omission does not affect the general subject-matter, but it contains data which one would like to know.

Children Handicapped by Cerebral Palsy. By ELIZABETH EVANS LORD. New York: The Commonwealth Fund; London: Humphrey Milford. 1937. Pp. x+105. 5s. 6d. net.

Dr Lord examined and followed up more than 300 cases of cerebral palsy, or 'birth injury', as it is popularly called. She here presents her results, mainly with reference to problems of the management and early education of the cases studied, writing in a direct, concise and excellently untechnical manner. The result is a very good monograph and one which has a wider application than the title of the study would suggest. She presents first some of the psychological aspects of muscle training, and makes some wise remarks concerning over-hasty attempts to force a child beyond his physical capacity. Next follows a study of the mental testing programme as it was worked out in this research, and what is said about the uses and limits of the technique adopted has application beyond the cases to which the tests were applied in this instance. There follow chapters on feasible educational plans in relation to mental development; on the teacher's problem, and some particularly apposite remarks on the emotional problems of child and parent. A bibliography is included. The book is one for the library or for the person with special interests. At the same time it has general psychological bearings and is a remarkably good study of its kind.

Factors Determining Human Behaviour. Harvard Tercentenary Publications, 1. Harvard University Press; London: Humphrey Milford. 1937. Pp. viii+168. 10s. 6d. net.

Here are eight lectures delivered in honour of the tercentenary of Harvard University. They are: The Nervous System, by E. D. Adrian; Hormones in Relation to Human Behaviour, by J. B. Collip; Principal Factors Determining Intellectual Evolution from Childhood to Adult Life, by J. Piaget; Psychological Factors Determining Human Behaviour, by C. G. Jung; Psychological Strength and Weakness in Mental Diseases, by P. Janet; Logic, by R. Carnap; An Example from the Evidence of History, by A. L. Lowell; and Culture as a Determinant of Behaviour, by B. Malinowski. It will be seen that each contributor is an outstanding expert in his own subject. This gives the book strength in that everything that is said can be said with the authority of admitted and wide knowledge, but it also means that most of the lecturers say in brief what they have already said more fully and perhaps more adequately elsewhere.

Movement and Brain Action. By HELEN COOMBER. London: Faber and Faber. 1937. Pp. 196. 3s. 6d. net.

In several ways this is an excellent book. The first chapter is called 'Mainly Physiological'. It describes the general physiological mechanism underlying muscular innervation, sketches the growth in the infant of co-ordinated movement, and indicates the characteristic movements of early infancy. The second chapter is an attempt to show how the early maturation of the sensory nerves and of movement may be utilized in early education, and may directly stimulate the growth of important cerebral patterns. Much of the rather detailed physiology here is of necessity somewhat speculative, but the author knows well that this is the case, and whether her explanations are right or wrong the practical value of her advice remains. The third chapter tries to show how certain types of movement, particularly those of some of the smaller sense organs, may indicate particular sorts of neuronic activity. Again, whether or not some of the detailed explanations are accepted makes little difference to the value of the writer's clear differentiation of desirable from undesirable movements in children. A very interesting chapter follows on 'Postures Indicative of Brain States', though in fact the 'brain states' are described, as indeed they must be, in psychological language. Specimen Reports and Exercises conclude the book. The writing is clear throughout and there are some very good diagrams. As an observational study of 'expressive movement' and of its significance for early forms of education, the volume is one which should attract attention and stimulate further research.

Nature Hits Back. By MACPHERSON LAWRIE. London: Methuen and Co., Ltd. 1936. Pp. 180. 5s. net.

In this little book the author discusses the illnesses of the street—despondency, lassitude and nerves. These are the "essential tragedy of modern life... more tragic than... cancer... consumption... and insanity". He maintains that despondency and lassitude are the result of congestion due to improper nutrition, and that nervousness is nerve exhaustion, produced by excessive physical work, noise or worry, which would not be exhausting if the individual had proper nutrition, i.e. natural food, pure air, water and adequate exercise.

Glands also have their effect and "nervous disquietude will persist until we appreciate the significance of glandular activity and base nervous disorder upon a physical and commonsense foundation".

There is a chapter on the child, in which nutrition is again emphasized, though the effects of spoiling and of belittling the child are also discussed; and another chapter, headed 'Sex', which though good so far as it goes is much too circumscribed

in its scope. One can understand the omission from this chapter of any reference to infantile sexuality; but many of the sources of despondency, lassitude and nerves are due to the sex difficulties of the adolescent and of the unmarried adult. Yet there is not a word about them. If these difficulties are removed the nutrition of the average individual is sufficiently adequate for him to lead a happy and healthy life, unless other difficulties, connected with work or with other vital interests, arise; for example, failure in an important examination, the loss of some beloved person, or of the means of livelihood, etc. Then no amount of proper nutrition alone will suffice. Some way out of the difficulty or some reorientation of the individual is necessary.

The author evidently has his thesis—that many common ills are due to faulty nutrition—very much to heart. But he exaggerates and spoils the force of his argument by a sensational, journalistic style. This is the kind of book which is not of much use. It frightens those for whom it would seem to have been written, for it increases the despondency of the despondent and the nervousness of the nervous; while it can be of little value to any practitioner.

Commonsense and Psychology. By ALAN MABERLY. London: Frederick Muller, Ltd. 1936. Pp. 160. 5s. net.

Much is written in the name of 'commonsense'. Here the word means merely a discussion, free from sensationalism, of various practical problems of adjustment in which a large number of people are interested. The author, as he says, is writing "for the reader who has an interest in the working of the mind and in the contribution the knowledge of such can make to the solution of his own personal problems and to the social problems of the day". The book is really a series of short essays on a variety of topics: self-knowledge and its value; mental mechanisms; some psychological types; sex; parenthood and the child; will and imagination; popular illusions about right, truth and time; intelligence and education; death; the conquest of fear and guilt. Generally speaking the views expressed are sensible, and may help to remove a good many common sources of worry. The brevity of the discussion sometimes leads the author to adopt a dogmatic style, but taking it as a whole the book fulfils its purpose very well.

That Inferiority Feeling. By JOHN S. HOYLAND. London: George Allen and Unwin, Ltd. 1937. Pp. 256. 7s. 6d.

Dr Alfred Adler has inspired this book. It gives numerous illustrations of inferiority feeling, and its alleged effects in everyday life and in regard to large economic, international and religious problems. The first, and longer, part is concerned with Causes and Conditions, and the second, and more 'uplifting' part, with Cure. Mr Hoyland avoids technicality and includes a considerable number of very excellent stories. Most psychologists who read the volume will be likely to find it rather long, rather dogmatic, and, in many cases, given to questionable interpretations. But the author has written in high spirits and with great confidence and it is possible that a good teacher would find his book stimulating for use in a study circle composed of members without very much psychological training but with a critical attitude.

The Background of Spiritual Healing. By A. GRAHAM IKIN. London: George Allen and Unwin, Ltd. 1937. Pp. 224. 6s. net.

This book is made up of lectures dealing with the relation of psycho-therapy to spiritual healing, as carried out through unction and the laying on of hands, and through confession. It fulfils a useful purpose in so far as it stresses the importance of some understanding by the healer of the underlying psychological causation of the symptoms he endeavours to relieve. But it should have been made more clear to the general reader that a knowledge of psycho-pathology and psycho-therapeutic method as superficial as that given by a book of this type is valueless to the healer except in so far as it leads him to further study.

Geneza ocen Niedorzeczności (Pourquoi considerons-nous certains textes comme absurdes?). Par TADEUSZ TOMASZEWSKI. Les Travaux de l'Institut de Psychologie de l'Université Jean Casimir à Lwow, Pologne; Tome I, Nr. 3. Lwow. 1936. Pp. 52.

The author endeavours to show experimentally, by obtaining the reports of various subjects on various texts, that 'absurdity' is a subjective rather than an objective quality. The judgement of absurdity is made when the reader is unable, after prolonged effort, to understand the text he reads, and attributes this failure, not to himself, but to the nature of the text. But such a judgement is made when the text does not conform to the reader's preconceived 'intent' that the text should possess certain formal traits, which intent differs with different readers and in different circumstances.

Art and Prudence: A Study in Practical Philosophy. By MORTIMER J. ADLER. New York: Longmans, Green and Co. 1937. Pp. xiv + 686. \$5.00.

Here is a book which would have had far more force if it had been severely pruned. Nobody would gather from the leading title that its main study is the art of the modern cinema. The author casts an exceedingly wide net, linking up his special problem with the more general topic of the relation of art generally to the State. Part I thus studies Poetry and Politics and discusses the views of Plato and Aristotle, and Christianity and Democracy, in a more or less historical fashion. The other three parts of the book deal respectively with The Motion Picture as Popular Poetry, with Science and Prudence and Art, and with Cinematics. From all this it will be gathered that the invention of titles is not a very strong part of the author's outfit. Science and Prudence, for example, consists really of an attempt to evaluate recent research on the influence of the modern motion picture. Much of what the author has to say on this matter is most interesting. On the whole he is, no doubt rightly, very critical of the mass of recent research, pointing out that though it pretends to scientific impartiality, much of it is little more than summarized popular opinion. Generally speaking Prof. Adler is favourable to the development of the cinema, considers that much of its alleged evil influence does not really exist, and endeavours to indicate how the better features of cinema production may be encouraged.

The book is verbose in style and its problems are insufficiently concentrated, but any psychologist interested in the effects of the modern cinema will have much to learn from the analysis and description of such research that has been done on the matter. He will also find material to set him thinking about methods of social research in general. But it is unlikely that he will gain very much from the discussions with regard to improved concrete methods of research.

Whether the cinema producer will learn anything much from the discussion of the criteria of good and bad cinematic art which is attempted in the final section of the volume is a matter concerning which the present reviewer can form no expert opinion.

Shakespeare and Psychology. By CUMBERLAND CLARKE. London: Williams and Norgate. 1936. Pp. 192. 7s. 6d. net.

The method of this book is to take some particular class of Shakespearean play, such as the Tragedies, or the Romances; or some particular type of character, such as the heroines and the fools; or some particular general topic, such as patriotism, national character, evil, and to write a chapter headed 'The Psychology of "so and so"'. The effort apparently is to show that Shakespeare had a profound knowledge of the working of the human mind, and to show that he anticipated many of the 'discoveries' of modern psychology. The style of the book is rather disconcertingly

jerky, and nothing is discussed very fully or to much purpose. The final chapter, which is called 'General Survey' and gives a quick-running account of the growth of psychology in recent times, is extremely inadequate. The book might have been better if it had covered less ground more thoroughly.

Religion and Reality: An Essay in the Christian Coordination of Contraries. By MELVILLE CHANING-PEARCE. London: Macmillan and Co. 1937. Pp. xv+190. 7s. 6d. net.

A Creed for Sceptics. By C. A. STRONG. London: Macmillan and Co. 1936. Pp. ix+98. 6s. net.

The Logical Structure of Science. By A. C. BENJAMIN. Psyche Monographs No. 9. London: Kegan Paul, Trench, Trubner and Co., Ltd., 1936. Pp. 344. 10s. 6d. net.

The Māndūkypanishad with Gāudapāpa's Kārikā and Śāṅkara's Commentary. Translated and annotated by SWAMI NIKHILANANDA, with a foreword by V. SUBRAHMANYA IYER. Mysore: Sri Ramakrishna Ashrama. 1936. Pp. xliii+361. Rs. 2-80.